USES AND LIMITATIONS OF SYSTEMS ANALYSIS

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by

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ABSTRACT

This is a study of the role of systems analysis in the strategic planning decision process of an organization. Its focus is on the interaction of systems analysis with the broader decision process, including the bargaining environment. Two case studies are included from the Department of Defense, but the thesis as a whole is applicable to commercial and non-profit organizations as well as governmental.

Decision-making consists of more than simple choice from a set of known alternatives. It also involves definition of objectives, laying out the structure underlying the decision, generating alternatives, and evaluating the alternatives in terms of the objectives. Since strategic decisions are frequently made in the face of complexity and rapid changes in the environment, the lack of a firm structure for the decisions (conceptual uncertainty) requires that the decision process be an iteration of these activities rather than a linear progression from given goals to final choice, and an approximately rational rather than deductively rational process. Systems analysis is a way of addressing strategic decision problems that emphasizes explcitness, quantification wherever appropriate, recognition of uncertainties, and sharpening -- rather than replacing -- the judgments of the decision-makers. It tries to find significantly improved alternatives by improving the understanding of the structure underlying the decisions.

Behavioral theories of decision-making in organizations have developed largely independently of economic and operational theories of how the organization's operations should be organized and how resources should be allocated. They have tended to focus on the mechanisms of bargaining and on how various human factors affect productivity. More recently behavioral approaches to decision-making have been investigated that are more relevant to the strategic decision process.

This thesis attempts to bring together these two largely separate types of considerations in order to understand better how systems analysis will interact with the broader aspects of the strategic decision process. A number of postulates are developed from the simultaneous consideration of these two areas. They relate to how systems analysis may be used by the participants in the decision process, its limitations in meeting their needs, and why use of systems analysis may create adverse reactions within the organization as well as improve the basis for decision.

The evolution of the decision process is documented for two strategic decisions in the Department of Defense: the decision to make CVA-67 a conventionally powered attack carrier, and the decision to ask Congress for a large construction program for Fast Deployment Logistics ships. The uses of analysis by the participants in the decision process and the reasons why the analysis evolved as it did are emphasized. These two cases are interpreted along the lines developed for how systems analysis and behavioral factors will interact in the decision process. It is concluded that the impact of conceptual uncertainties on the decision process were underemphasized; decisions on how to proceed with analysis and how to use it in dealing with others are relatively more tentative and less firmly aimed at well-defined purposes than postulated at the outset.

Thirteen dimensions of the strategic decision process are suggested as a structure within which the interactions of explicit policy analysis and bargaining considerations can be described. These thirteen characteristics are: structure, incrementalism, relativism, and simplification; alternatives, commitment, and energy; expectations, motivations, information, and coalitions; and rationality and improvement.

Because of the extreme complexity of the relationships in strategic decision-making and uncertainties about them, the most useful role of theories of strategic decision-making is to suggest, as this thesis has done, insights that analysts and decision-makers can assimilate to improve their "feel" for dealing with such situations.

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CHAPTER I

Introduction

This is a time of ferment in concepts for the management of large organizations -- industrial, governmental, and non-profit. The rapid rate of social and technological change since the war has made it increasingly difficult for them to adapt their activities through the trial-and-error process of learning from experience. At the same time, many organizational operations -- especially industrial -- have been sufficiently stabilized through the standardization inspired by the scientific management movement of 50 years ago to permit our understanding to develop to the point that rather sophisticated models can be applied. This modeling approach to operational problems, loosely grouped under the labels of operations research and management science, has permitted some significant improvements in the way organizational operations are carried out. Because these techniques deal with how operations are to be organized and performed rather than with the technology and the work itself, it is not surprising that they came to be associated with management. And it is not surprising that management and operations researchers have sought to extend the use of these techniques to the problems of managing an organization as a whole.

Largely in parallel with this movement has been the tenure of Robert S. McNamara as Secretary of Defense, with his revision of the top-level decision processes of the Department of Defense. The techniques of program budgeting and explicit analysis of resource allocation decisions -- usually referred to as systems analysis -- have figured importantly in Mr. McNamara's management style. This is one of the most thorough and imaginative attempts to improve the organizational decision processes at the top of a large organization that is faced squarely with the change, complexity, and uncertainty that is becoming increasingly typical. It certainly is the best known such attempt.

This is the background for this study of the role of analysis in the top level decision processes of large organizations and in particular the Department of Defense.

A. The Decisions

We clearly are talking about a small subset of the decisions that are made in the organization; we are even talking about a limited subset of the decisions that are made by the members of the top management group of the organization. Probably the best description of the class of decisions that we will be concerned with is given by Robert Anthony in Planning and Control Systems [1]. He categorizes the functions of management into five areas: strategic planning, management control, operational control, financial accounting, and information handling. These areas are described as follows:

Information handling is the process of collecting, manipulating, and transmitting information, whatever its use is to be.

Financial accounting is the process of reporting financial information about the organization to the outside world.

Operational control is the process of assuring that specific tasks are carried out effectively and efficiently.

Management control is the process by which managers assure that resources are obtained and used effectively and efficiently in the accomplishment of the organization's objectives.

Strategic planning is the process of deciding on objectives of the organization, on changes in these objectives, on the resources used to attain these objectives, and on the policies that are to govern the acquisition, use, and disposition of these resources.

Within this framework, we will consider only those decisions of management that fall into the strategic planning category as it is defined in Anthony's framework.

Strategic planning "connotes big plans, important plans, plans with major consequences" [2]. The decisions made in strategic planning are those particularly significant decision problems that have to do with the character of the organization over the reasonably foreseeable future. Under this definition, strategic planning decisions include only part of the planning decisions of the organization and include some types of decisions that may not be thought of as planning. We are

not concerned with the decisions involved in laying out detailed plans for the achievement of organizational goals, but with the decisions in rather aggregate terms about which goals to pursue and what resources to commit to each. On the other hand, we are concerned with those apparently current decisions that will significantly constrain future alternatives available to the organization or that will significantly affect the environment within which the organization will operate in the future.

For example, a firm's decision on the location and size of new distribution facilities is considered a strategic planning decision, but the decisions necessary to obtain those facilities efficiently would not. The decision about whether an aircraft carrier should be powered conventionally or by a nuclear plant is a strategic planning decision because of the long term impact of that decision on the characteristics of the fleet and the small number of carriers in the fleet. The much more complicated defense planning problem of how to organize and phase the logistics for a theater of operations would not be considered a strategic planning problem, but rather an operational problem; it is a vitally important problem, but it is not a part of the problem of charting the future course of the Defense Department.

Simon [3] has used the programmed-unprogrammed categorization of decisions. While this is a useful concept to which we will refer from time to time, unprogrammed decisions are not synonymous with strategic planning decisions. This and other categorizations of decisions will be discussed in detail in Chapter II.

B. The Need for Analysis

Probably the major impetus for improved analysis of strategic planning problems is the increased rate of technological and social change that has come about since the industrial revolution. A close second is the increasing complexity of the systems with which the strategic planner must concern himself. If the rate of change were relatively slow, the organization could adapt almost unconsciously via successive and small increments. A slow rate of change gives the organization time to "feel out" the environment through conscious experimentation or through observation of the effects of random influences; only marginal information is needed for the organization to adapt. Of course, it may not adapt optimally with such narrow views of the way it interacts with the environment; but as we will see, optimality is neither the great concern of the people who manage these organizations nor is it a particularly operational concept at the strategic planning level of the organization.

If the rate of environmental change is sufficiently slow, the complexity of the organization or of the environment does not alter the above view. Just as the engineer linearizes complex non-linear systems to make his problems amenable to his analytic tools, the manager in the face of complexity limits his consideration to such small incremental policy changes that he can be reasonably sure he is moving in a desirable direction. Of course, complexity does make adaptation more difficult. The manager has only a limited amount of time and only limited

capabilities of thought. In a relatively simple environment, he will learn from experience which are the most important relationships and slowly move toward more efficient operation.

It is interesting to note that these are the conditions assumed in classical economic theory and are the implicit assumptions behind most arguments for pure competition. Under the profit incentive, each firm is assumed to find through experience its most efficient operating conditions. This is essentially a static equilibrium viewpoint, and change is considered only with respect to how one equilibrium compares to another. How we would extend the concept of pure competition to a rapidly changing economy that is complex beyond the comprehension of those managing its components is not clear. It is clear, however, that the current tendency toward large corporations is due in part to causes different from those at the turn of the century. Much of the aggregation today is due to each firm's desire to stabilize the environment within which it operates. As a result, many mergers seem more pathetic than pernicious.

Bureaucracies are notorious for adapting to change and complexity by refusing to moved by either, adopting policies that are relatively insensitive -- if unresponsive -- to change and complexity. Even in less extreme cases, both industrial firms and government departments seek to stabilize and simplify their environments by shaping the terms on which they deal with their suppliers, constituents, and customers. Both seek to create demand for their services qualitatively as well as

quantitatively in order to ease the demands placed on the organization. Such devices go only so far, however, and increasing change and complexity mean that organizational performance lags behind the pressures for adaptation. Equilibrium with the environment is always just out of reach. It is here that the need for analysis rises sharply at the policy level of the organization -- when the rate of change is not slow relative to the time span of strategic planning decisions, when past experience alone is inadequate, and when the complexity of the decisions exceeds the unaided capabilities of the decision-makers.

The complexity of the systems involved in strategic planning decisions may be either technical or interactive. The science and public policy debate is an example primarily of the former -- where the decision-maker cannot know much of the technical substance about which he must decide. More commonly, the technical details that are relevant to strategic decisions can be sufficiently well-understood by the decision-makers. It is the complexity of relationships among the various elements of the decision that make some form of analysis important.

In addition to change and complexity, other factors have made analysis increasingly necessary for strategic planning decisions. Increasing affluence in the economy means that more and more options are available to the firm and to the government. So long as the goal of the organization is simply "more" of something or striving to overcome some difficulty, priorities are relatively easy to set and

decision-making is essentially synonymous with problem-solving. But when we must choose goals as well as the means of reaching them, the decision process becomes much more complex. A by-product of both technical and interactive complexity is the increasing specialization of knowledge. It has become increasingly difficult to bring together all the information relevant to strategic decision problems because so many specialties are involved that no one person has a good grasp of all the relevant information. Both the profusion of goals and the increasing specialization have also increased the need for analysis in strategic decision-making.

C. The Reason for this Study

Given that analysis is becoming increasingly desirable for strategic decision problems, we need to understand better how explicit analysis relates to the wider decision process. In spite of the successes of some analyses, our current knowledge of how to analyze strategic decision problems and how to fit them together to shape the future of the enterprise is slight. The theoretical foundations are weak and the principles or rules of thumb that have evolved are not alone specific enough to assure good analysis. In order to improve this situation, we need to improve our understanding of the role of analysis in the strategic decision process of large organizations, and the best way of doing so appears at present to be a study of that process in organizations where its application has been well-developed.

President Johnson's introduction of the Planning-Programming-Budgeting System (PPBS) for the Federal budget process beginning with the Fiscal Year 1968 budget provides a special impetus for a better understanding of how to fit analysis into the governmental decision process. While the successes of program budgeting and systems analysis in the Department of Defense under Secretary McNamara have been widely reported, there is little indication that the actual role of these tools in the wider decision process has been as widely appreciated. The strife between the Secretary and the Congress and the armed services has often focused on systems analysis in a way that indicates that the latter two institutions fail to appreciate the role of explicit analysis in making strategic decisions and that none of the parties fully appreciates the implications of heavy reliance on explicit analysis of policy issues in a political decision process.

Operations research has often been described as a set of tools in search of problems, but the problem we are addressing here is the opposite. The theoretical tools of operations research, economics, organizational phenomena, and political theory are contiguous but tangential to the question we are trying to address: <u>what is the role of analysis in the strategic decision process of an organization and what are the effects of analysis and the decision process on one another</u>? What we must do is to examine the relevance of each of these bodies of theory to the problem and to devise a synthesis of some sort to give us some tools and concepts for talking about the use of analysis in the strategic decision process.

How can this be done? One way would be to pull each of the related bodies of theory into some common structure. The problem is so complex, however, that this is likely to be an exercise in conjecture. The traditional "scientific" approach of studying the actual process and uncovering repeatable regularities is essentially impossible. There are so many relevant variables and so few cases where analysis is actually being used in the strategic decision process that a directly empirical approach would be of little use. What is called for is a pragmatic combination of conjecture based on reasonably acceptable principles drawn from the theoretical areas and on correspondence of those principles with observations of actual situations.

This thesis is a study of the uses and the limitations of systems analysis with special attention to the experience in the Department of Defense. These uses and limitations have been traced in two particular decisions: (1) whether the propulsion for the attack carrier CVA-67 should be nuclear or conventional and (2) the decision to ask Congress for authority to build the Fast Deployment Logistics (FDL) ships. The result is a mixture of essay, empiricism, and theorizing. It is not elegant in its formulations, nor is it comprehensive in its explanations. Hopefully, however, it will be useful to students of systems analysis and organizational behavior by suggesting some directions and concepts for a theory of analysis in organizational strategic planning. And hopefully it will be of use to those who work in the strategic planning

role by suggesting some insights into why analysis and people interact as they do and how analysis might be made more useful in the light of these factors.

D. A Brief Overview

Chapter II describes the strategic decision problem in detail. Various concepts of planning are discussed and the relation between specific decisions and the overall strategy of the organization is explored. Systems analysis and program budgeting are described and related to the strategic planning problem.

Chapter III outlines and discusses various theories of organizational behavior. Particular attention is given to those aspects that are relevant to strategic planning and systems analysis. Chapter IV compares the organizational behavior views and the strategic planning views of the decision process to suggest where they are compatible, where they conflict, and some implications of considering the two in a common format.

Chapter V describes the CVA-67 decision and Chapter VI the FDL decision. The emphasis in these descriptions is on the development over time of the decision process and in particular the uses of systems analysis made by the principal participants in the decision process in their dealings with one another. Chapter VII discusses each of the decisions in terms of the theoretical foundations of Chapter IV from

two viewpoints: how specific aspects of the decision process can be interpreted and how various theoretical concepts are or are not borne out in each case. Some suggestions of how we might synthesize the various areas of theory are also included. Chapter VIII suggests some implications of this study for future research into the strategic decision process.

CHAPTER II

Strategic Planning and Systems Analysis

The words "decision" and "planning" encompass a wide range of concepts and modes of behavior. We decide what to do on Saturday night; we decide which stocks to invest in; we decide how to get dressed in the morning; we decide how to fill a sales order; and we decide who will win the next presidential election. Each of these is a decision, but the cognitive activity involved in each is considerably different from the others. The first is a preference choice; the second a resource allocation; the third, a habit; the fourth, problem-solving; and the last, a judgment about some unknown fact.

Similarly, there are many types of planning. We plan the Apollo moon-shot; we plan a party; we plan our careers; and we plan a football defensive strategy. The first is a detailed laying-out of the steps to be taken and in what order in order to reach some well-defined goal. The second is a less detailed laying-out of steps with more flexibility in their makeup and order. The third is a much less detailed tentative guide to decisions that will be made in the future as circumstances arise. The last is a moderately detailed prescription of action for a number of possible situations.

In this chapter we discuss various aspects of planning and decision-making to give a richer and more precise terminology. Then a brief discussion of what is meant by "rationality" and "objectivity" in decision-making is given. Finally, systems analysis and program budgeting are described as they are conceived and practiced today.

A. Decisions

Anyone interested in studying decision-making would do well to start by reading The New Science of Management Decision [4] by Herbert Simon. It provides one of the first and the best discussions of the scope and structure of the many activities we call decision-making. Most previous writing on decision-making was concentrated in two areas: the psychologists' study of how people solve problems and the "wisdom" literature of business administration. The former tended to concentrate on measurable aspects of problem-solving behavior on simple tasks in a laboratory setting. It is not surprising that neither management theorists nor managers have found this body of literature of much use. The "wisdom" literature, on the other hand, consists of glowing generalities alternated with pregnant anecdotes that offer little concrete substance. These men of experience made their successes through good judgment, and more often than not they gave the impression that good managers were born, not made. Their decision process was largely the exercise of undifferentiated -- but seasoned -- judgment and was treated as something mystical at the very least.

Dewey [5] attempted to look objectively at the human thought process around the turn of the century (coincidentally at the same time Taylor was developing "scientific management"). He characterized the problem-solving process in five stages:

1. Suggestion
2. Intellectualization
3. Hypothesis
4. Reasoning
5. Verification

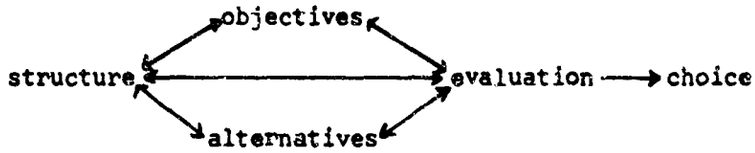
The processes of intellectualization and reasoning, of course, remained beyond much specific description.

Simon's characterization [6] of the decision process involves three stages: intelligence, design, and choice:

> Intelligence activity sets the stage for a strategic decision by discovering a problem in need of solution or an opportunity available for development. In general, intelligence activity involves scanning the environment and collecting information on various trends.
>
> Design activity begins once the area of action has been determined by intelligence activity. At this stage, alternative means of solving the problem or of exploiting the opportunity are developed, and these alternatives are evaluated.
>
> Choice activity is concerned with choosing one from the alternatives that have been developed and evaluated. The "integration" of the various strategic decisions into a unified strategy is included in this category.

For a discussion of the use of analysis in the decision process, a slightly different characterization is useful. It involves five activities: goal or objective determination, structuring the relationships among the variables, alternative generation, alternative evaluation, and choice.

But the strategic decision process is not linear as these characterizations might suggest; in any given decision, we may jump from one phase to another many times before finally taking some concrete action. Rather than just list the five phases, then, we can diagram them:



This emphasizes the fact that a strategic decision problem is not an exercise in logical deduction, but rather an iterative process involving the interaction of goals, alternatives, and our understanding of the problem itself.

Objectives are usually taken as the starting place for decision-making when it is viewed as problem-solving. To be "rational", we have to know what we want to achieve, and economics and management science both assume the decision-maker knows what he wants. If this were true, strategic decision-making would indeed be an exercise in problem-solving. Conceptually, we would need only to array all the alternatives,

evaluate them in the light of the environment, and select the one that best achieves our goals. This is not a reasonable prescription for practice, however, for two reasons: 1) it ignores the limitations on our cognitive capabilities in generating and evaluating alternatives, and 2) it assumes that we not only know what we want, but in detail how much we are willing to give up of something else. While we are discussing objectives, let us examine the second point.

At any point in time, each person has a collection of objectives or goals. These range from the very basic long-term and continuing goals to strictly temporary minute-to-minute operational goals. Bentham [7] approached the problem of utilities by starting from the basic goals of seeking pleasure and avoiding pain. More recently, Maslow [8] has postulated the needs-hierarchy concept, where "higher" needs are activated only as "lower" needs are satisfied; his hierarchy runs from physiological needs through safety needs, social needs, and ego needs to self-fulfillment needs. Both these very basic views may be said to refer to motives for choosing goals rather than to goals themselves. Anyone who has played the game of "Why?" with an inquisitive seven-year old knows that we do in fact have a hierarchy of goals, each resting on some prior purpose.

In strategic decision-making, we are concerned with goals somewhere in the middle of the hierarchy. Day-to-day operational goals are far too numerous and transient to be considered as goals in the strategic planning context. And our underlying motives are

too far removed from the situation to be explicitly included. Where, then, do we find useful goals in a strategic decision problem: how do we decide what it is we want to do? The answer lies in the iterative nature of the decision process illustrated above.

What we want to do (goals) depends on what it is possible to do (alternatives), on our understanding of the environment in which we will do it (structure), and on our assessment of what it is the various possible actions will accomplish (evaluation). People can rarely formulate their preferences in the abstract; rather, they must ask: "Would I prefer this or that in this particular situation?"

March and Simon [9], Cyert and March [10] treat the question of goals by the concept of aspiration levels for each goal dimension that are based on historical experience. This is certainly reasonable in a relatively slowly changing environment, but it really rather begs the question since it states little more than that we always want "more" if we can get it. And this concept is really more applicable to the areas of management control and operational control than to strategic planning. Lindblom [11] takes the view that ends and means (or objectives and alternatives) are inextricably intertwined. In this view, the decision-maker assesses the benefits of each available alternative against the others, asking himself as he does so whether he is willing to give up so much for this for so much of that; it is through this process that preferences and tradeoff parameters are established rather than being posited <u>a priori</u>.

Such a view of objectives is, of course, foreign to the scientific or rational approach to decision as conceived by many. The management literature is full of comments on the elegance and rationality of the microeconomic theory of consumer choice and equally full of complaints that elegance and rationality unfortunately do not make the theory useful. It is not accidental that operations research techniques have found most application where goals were relatively well-understood, but where the problem-solving necessary to realize them was beyond unaided intuition or trial and error experimentation. Our Western 20th century concepts of rationality strongly suggest a deductive reasoning process for decision-making that can be proven to be "right" -- i.e., to be in accord with certain commonly accepted deductive axioms. Given this view, it is not surprising that observers of management behavior have found that most decisions in an organization arise in the form of problems. As Pounds [12] has observed, the process of problem-finding is largely one in which problems find decision-makers and to which the deductive problem-solving approach is applicable -- in spite of the voluminous "wisdom" literature in management advocating a positive approach of "management by objectives". (In probably the most advanced work of this type, Ansoff prescribes a procedure for arriving at a set of goals for a firm. While his scheme is eminently reasonable on a conceptual level, it leaves too much of a gap between the concept and the execution; it is not operational.)

Many will regard this view of goals and objectives as defeatist; others will recognize it as rather existential. The so-called rational approach recollects the Newtonian mechanistic view of the universe: if only we could fix the position and velocity of each particle at some point in time, we could predict everything that would happen in the future. Similarly, once we can determine our goals, we can by a rigorous problem-solving process (scientific and rational, of course) make decision-making a true science and remove it from the vagaries of subjectivity and caprice.

It did not take too long for scientists to accept the fact that such a Newtonian computation was hopelessly beyond our capabilities. Yet the concept hung on: The uncertainty principle upset even Einstein -- not because it manifested itself in daily life, but because it cut out from under physics a principle so implicitly accepted that it pervaded our very way of thinking. The concept of goals advanced here is hardly so profound as that of the uncertainty principle, but it does go beyond the conclusion that analysis for strategic decision-making is limited solely by our computational abilities. Even that conclusion has been slow in coming to the theory of decision in management and economics, but it did not deter the view that ever more powerful computational techniques would permit at least an asympotic approach to rational and scientific decisions even in strategic planning.

In short, goals or objectives are derived from our understanding of the environment and the alternatives open to us. By goals and objectives we mean the dimensions along which benefits are measured and the distance along those dimensions that available alternatives will take us. We are talking about decisions rather than aspirations and although the words goal and objective are often used to mean targets we hope to achieve, that meaning will not be used here. As used here, goals and objectives are the measures by which the decision-maker assesses the alternatives to arrive at a choice. We will have more to say about this when we discuss the role of judgments in decision-making.

The structure of a decision is simply the relationships of the relevant variables to one another and to the environment. Some of the variables are control variables in the sense that the decision-maker is relatively free to specify them independently of other variables. Other variables may be considered as measures of effectiveness or objectives, and still others that are neither directly controllable nor particularly important in themselves can be called intermediate variables. This should not be taken to mean that we are referring only to quantitative or even quantifiable factors; it is simply a convenient terminology for referring to various substantive -- as opposed to relational -- aspects of the decision.

The structure of a decision problem may be very simple or very complex, and the relationships may be susceptible of very explicit description or only very generally known. To a large extent the complexity of the structure is determined by how complex it is worth being. Many practicing managers would argue that much of the theory of management is unnecessarily complex; certainly many would feel that this particular work belabors the obvious. Indeed, any decision can be made hopelessly complex by bringing in factors of less and less importance. Nevertheless, in strategic planning for an organization in a rapidly changing and highly-interconnected environment, more and more factors need to be taken into account in order to make good decisions.

Quite independent of complexity is the susceptibility of the pertinent relationships to explicit statement. The relationships among the millions of components in the circuit of an electronic digital computer can be described very explicitly and in great detail by electronic engineers, but the computer so viewed is so complex that such a description would be of little use.

The structure of the problem really refers to our understanding of the problem. It consists of the definition of variables and their relationships to one another and to the boundaries between the problem and the environment. It provides the context within which the impact of variables on one another are assessed. In this sense, it makes the difference between data and information -- for numbers make sense only in a context. It is the key to defining our objectives, generating alternative courses of action, and evaluating the alternatives. In short,

it is the relationships among ends and means. When we have trouble defining our objectives in a situation and when we find it difficult to find a basis for comparing alternative actions, it is an indication that our understanding of the structure underlying the decision is lacking.

Alternatives need little discussion; we all know what alternative courses of action are. It is useful, however, to discuss the generation of alternatives. Two points in particular should be made: (1) alternative generation is difficult, and (2) it is tied up with the definition of our goals. It is too tempting to a student of operations research to think of alternative generation in terms of listing all possible combinations and permutations of a set -- for example, the set of all feasible points in a linear programming problem. In fact, alternative generation is a creative act and just about as simple as creating a work of art: it is easy to think up useless alternatives, but it is exceedingly difficult to come up with a truly creative one. It has been said that one or two good ideas in a lifetime is enough, and the same applies to alternatives in strategic planning. Good alternatives are hard to come by because no one has thought of them before, and it is very difficult for the human mind to think of things it hasn't thought of previously.

We have already touched on the relationship of alternative generation to the definition of our goals: what is a good alternative is largely determined by what we want. The reader may recall that in

our discussion of objectives, we said just the opposite. The fact is that the two are intertwined. Where we stop in this iterative definition of objectives and alternatives is largely determined by the decision that the cycle has reached the point of diminishing returns when considered against the omnipresent alternative of postponing action. Simon distinguishes two types of goals: test goals and generating goals. That is to say, improved performance along some of our goals is rather more important than others, and we focus our search for alternatives on those goals, relying on the evaluation process to test for the other goals.

The process of generating alternatives includes some evaluation. We implicitly screen out grossly inferior alternatives by simple tests of acceptability. Often these tests are based more on similarity to alternatives found acceptable in the past than on effectiveness, but this is simply a reflection of the fact that this is a pretty efficient screening device. It is seldom that a radically unfamiliar alternative proves useful because the alternative chosen must fit in with other areas of the organization that are not included in the decision problem at hand.

Evaluation is probably the part of the decision process most people have in mind when speaking of decision-making, and it is usually interpreted as finding the best solution to the decision problem. (Too often, new goals and significantly new alternatives are viewed as a change in the problem rather than as a part of it.)

It is in evaluation that computational ability is most relevant, and it is not surprising that this is where operations research techniques have found the most application. Such an interpretation of the evaluation process really confuses the separate but related tasks of determining the effectiveness of each alternative along each goal dimension and of choosing one of the alternatives based on that evaluation. It is the former process that we will call evaluation, and the latter will be called the choice process.

It is the evaluation part of the decision process that seems to be the basis for the problem-solving school of decision-making, and it appears to be the image of decision-making most economists and operations researchers have in mind when advocating more analysis and rationality in decision-making. Since it is the process of tracing through the consequences of an action that is most amenable to the deductive tools of rational and scientific analysis, it is not surprising that this part of the decision process has attracted the attention of these professionals. Further, there have been a number of decision areas in the firm that have been fairly well understood qualitatively and where the most interesting alternatives were understood but could not be evaluated effectively because of limited computational ability; it is mainly to these areas that operations research has been applied.

Evaluation is also intimately tied up with structuring the decision. Tracing through the consequences of an alternative involves tracing through the structure of the problem. In fact, it is usually

in the evaluation process that the impetus for improved structure or understanding of the problem arises. We are seldom directly aware that our understanding of the problem structure is poor: When goals are only vaguely defined or when interesting alternatives are not visible we often are not so much aware of a need for understanding the relationships as of a need for clearer goal statements or ideas for action. Unfortunately, even the evaluation process does not guarantee that we will seek better understanding of the structure. Analysts often become trapped in their techniques and fail to look for better-suited ones.

Choice is the final part of the decision process. It is the commitment to action, the culmination of the decision process. In this study, we will use choice in two senses: (1) the choice made by one of the principal participants in the decision process of the alternative he feels is appropriate, and (2) the choice finally taken by the organization. For now, we can concentrate on the individual decision-maker.

There is a phase somewhere between choice and evaluation that could be considered in either of the two categories; it is when the decision-maker decides whether he is ready to make a choice or whether more work is necessary in one or more of the four pre-choice phases. We shall consider it part of the choice process since that decision appears to involve the same thought processes as the actual choice itself.

An individual decision-maker's choice process is not very well-understood. It is basically the process by which he decides which alternative -- which mix of performance along each of the objectives -- he prefers. In the language of the economist, it is how he generates a portion of his utility function. In other words, he exercises his judgment based on the alternatives available, the results of the evaluation, and his perception of the environment. The word judgment has been used rather loosely in the management literature and generally. Judgment is exercised throughout the decision process, but it is the choice process that is most often referred to in management decision-making.

We will distinguish two types of judgment made by the decision-maker in the choice process: judgments of fact and judgments of value. Factual judgments can be about the value of some parameter or the degree to which some condition is met; for example, the demand for a new product or the degree of flexibility in some logistics arrangements. Factual judgments can also be about relationships between quantities or between phenomena; for example, how the added range of a nuclear-powered carrier contributes to the military effectiveness of a carrier task force or what factors determine the amount of medical care some population will seek.

Value judgments are the decision-maker's judgments about his preferences for the performance mix of the alternatives. Why he prefers one set of performance measures over another, factual

judgments equal, will be considered beyond the scope of this paper; for our purposes, he just does. We will be concerned with the process by which he clarifies his preferences through information from the four pre-choice phases of the decision process.

Of course, fact and value judgments are intertwined in any real decision; only conceptually can we isolate the factual and the value components of a decision problem. Some of the objectives formulated in the decision process are objectives within themselves. Others are stated as objectives because there is no way to formulate the factual relationships among them; we rely on the judgment of the decision-maker to discern the effectiveness of each combination of such measures. (Conceptually, if we knew the structure of the decision problem fully, these surrogate measures would not be necessary, and only those measures that required value judgments for their tradeoffs would need to be presented in the evaluation.)

It is very much the contention of this paper that strategic decision problems arise in the context of a dynamic environment made up of elements within the organization and without. Thus we hold that there are a myriad of variables and factors that impinge on each strategic decision. All of these cannot be included in the explicit decision process, not even all the significant ones. The choice, then, will hinge not on the results of the formal evaluation alone, but also on many of the second-order factors the decision-maker is aware of that are not included in the explicit evaluation.

B. Classifications of Decisions

There are many different ways decisions can be classified: by subject, by who makes them, by the number of participants, by the amount of knowledge, by the riskiness, by the size of the consequences, by the time span affected, by the time available for analysis, by the extent to which a programmed decision rule applies, and on and on... The strategic decision problems of an organization inherently involve a number of participants -- they are group decisions. And they involve, by definition, large consequences. Beyond these two characteristics, strategic decisions vary widely on the other dimensions. We will discuss here only the most important.

Perhaps the most important characteristic of strategic decisions from the standpoint of analysis is the extent to which the structure of the decision is known. The distinction is often drawn between structured and unstructured decisions, implying a dichotomy. Of course, these merely represent the extremes. Any real decision will lie on the spectrum between the two, but just where is much more nearly a subjective feeling than a demonstrable fact. It is generally characteristic of strategic decisions that the decision-maker is uncertain about many (or most) of the relevant structural relationships as well as the parameters of the decision. We are on safe ground therefore in calling most strategic decisions unstructured, but it is worthwhile to remember that some are more so than others.

Another dichotomy often used in characterizing decisions is that of programmed vs. unprogrammed. "Programmed", however, can be used with two different meanings. It can refer to the timing of the decision, or it can refer to the procedures used to make the decision. With an annual budgetary process, major resource allocation decisions are programmed strategic decisions in the former sense of the word. Whether such decisions should be made on a programmed basis in this sense or as developments arise is the subject of considerable debate, but it is of minor importance to this discussion. We will use the latter sense of the word which is similar to its use in computer programming. In this sense, strategic decisions can be almost anywhere along the programmed-unprogrammed spectrum.

It is worth noting that programmed and structured are not synonymous concepts. It is quite possible for programmed decision rules to evolve for quite unstructured decisions. The programmed "two-bin" and "s,S" inventory re-order decision rules were in use long before operations research modeling stimulated improvements in the structure of inventory control decisions. We should expect a trend, however, toward less and less use of programmed decision rules as we progress toward increasingly unstructured decisions, and in fact this seems to be the case. With increased knowledge of the structure of operational decisions, decision rules have been applied to take such decisions out of the "art" category and make them programmed. Strategic decisions are relatively unstructured compared to these operational decisions and are still made largely on the basis of ad hoc management judgment.

It is also worth noting that decisions may be programmed implicitly rather than explicitly. Habitual patterns that evolve for dealing with frequently encountered situations are actually programmed decisions, even though the program may not be recognized or acknowledged. Clarkson's demonstration that the investment portfolio selection decisions of an investment analyst could be represented by a set of decision rules that were implied by repetitively applied "judgment" is an excellent demonstration of this [13].

The last important decision characteristic we will discuss is more subtle than those above, but it is important to understand in talking about strategic decision-making. There is no convenient name to apply, but it is basically the distinction between problem-solving and shaping the future that was touched on above in the discussion of goals. There is much problem-solving activity in strategic decision-making. For example, tracing the implications of the alternatives for each of the goal dimensions is a problem-solving exercise. Further, the goals developed in a strategic decision are often proxy objectives for some higher but ill-defined objective; in these cases tradeoffs among the goals are partially problem-solving judgments and partly value judgments. But the essential point is that strategic decisions are basically decisions about what <u>will be</u> or <u>should be</u> rather than about what is objectively the <u>right</u> answer to a stated problem.

C. Planning

"Planning" must be a very close runner-up to "decision-making" as the most overused and least specific word in the literature of management and of political science. Planning implies preparation for the future and the projection of courses of action. As such it is hard to fault, and most organizations of any size have a group dedicated to planning. Just as we found many types of decisions, however, there are many types of planning and it is worth spending some time clarifying their distinctions.

Probably the most common conception of planning is the detailed fitting together of a number of activities to achieve some desired end. For example, planning the family budget involves the specification of just how much of the limited money goes to what activities. Planning the installation of a new facility involves making sure that each component arrives at the right time and place in relation to all the other components. The PERT technique owes much of its success to the fact that it formalizes this common conception of planning and provides a graphic aid for relating many sub-problems to a specific end.

This type of planning has been called "Cook's Tour" planning because of its emphasis on detailed specification in advance of each component activity in relation to all the others. This type of planning has one major drawback: it assumes that we know -- at least approximately -- what the pieces are to be fitted together, how they relate to one another, and just what are the goals we want to achieve. It is the clear correlate of the problem-solving approach to decision.

The other extreme in planning is "Lewis and Clark" planning. Here the goal is far removed from near-term activities, and proximate goals must be derived as the operation evolves and learns about its environment. It is an adaptive approach to planning rather than an optimizing approach. The uncertainty faced by Lewis and Clark was not unlike that facing a large corporation or government department in the rapidly changing environment of today. Some sub-activities can be Cook's Tour planned such as the loading of the longboats for the trip up the Missouri or the construction of a new manufacturing facility, but the planning for the operation as a whole can only be tentative and general. These two types of planning are essentially the two types of coordination mentioned by March and Simon: coordination by planning and coordination by feedback [14].

The concept of contingency planning has been developed to extend the rational ideal of Cook's Tour planning to an uncertain world. As such, it lies somewhere between the two extremes, but much closer to Cook than to Lewis and Clark. To do contingency planning, we still must know the possible outcomes of the pertinent events, how these outcomes affect other activities, and how they relate to our goals. Contingency planning has many parallels with the extension of mathematical optimization techniques from models under certainty to probabilistic expected value models; both are still high structured and far removed from situations of extreme structural as well as parametric uncertainty.

Just as we drew the distinction in decision-making between problem-solving on the one hand and the simultaneous choice of goals and alternatives on the other, a similar distinction applies between Cook's Tour planning and Lewis and Clark planning. Both strategic planning and management control, as they have been defined by Anthony, require planning, and each will involve a mixture of the two types. Strategic planning will necessarily involve more of Lewis and Clark, while management control will be able to make more use of the Cook's Tour approach. But this difference in the types of planning should not be confused with the distinction between defining objectives and devising ways to carry them out effectively. The difference is that of devising a way to go to the moon or devising a national space program.

It is clear that most of the strategic planning activity will be made up of Lewis and Clark type planning and largely ad hoc strategic decisions. This is almost the antithesis of rational decision-making as most people would interpret it; organizations are supposed to have a purpose or a strategy from which activities are derived rather than the other way around. This points up the distinction between grand strategy and evolved strategy in strategic planning and decision-making.

The grand strategy approach to strategic planning starts with organizational goals and assets, strengths and weaknesses, and proceeds in a deductive, problem-solving way to derive the optimum mix of activities for the organization. In the evolved strategy approach, major decisions are made as the need or opportunity arises and in terms

of the proximate goals as seen at the time; the long-term strategy of the organization thus evolves over time as a result of a series of specific decisions. Most of the management literature calling for more planning by top management is calling for the grand strategy approach; the evolved strategy approach, after all, resembles the muddling-through methods used by managers all along. Ansoff [15] has recently presented probably the clearest and most reasoned blueprint for grand strategy planning. It is an eminently reasonable approach, but it simply calls for too much information and computation to be practicable. This is the ultimate in what Lindblom has called the rational-comprehensive approach to decision-making, and it suffers from exactly the weaknesses Lindblom lists. It places unreasonable demands on the time and resources available to the organization for analysis, and it assumes a unique solution can be identified when, in fact, that is seldom the case.

This is not to argue that it is useless to think about the purposes of the organization and to assess its capabilities against the environment in which it operates. But it is a fact of life that such thinking is seldom very operational -- it is rarely feasible to link these considerations realistically to the actual decisions that must be made. This is just to say that in trying to improve the state of the art in strategic planning and decision-making, more progress appears likely at present by trying to make individual decisions more consistent with one another and more closely related through objectives,

than by trying to derive the decisions from rigorous a priori objectives. All this raises the question of just how we can define rational decision if we are denied the rationality of the deductive reasoning process from objectives to actions.

D. Rationality in Decision-making

As has been implied in the above discussions, much of our concept of rationality has been derived from the deductive method of reasoning that has proven so useful in natural science and mathematics. Examples of this include the micro-economic models of the firm and of consumer choice, the pejorative connotation of subjectivity as opposed to objectivity, and the search for a management "science". Both operations research and systems analysis arose from the desire to make decision-making more rational.

However, rationality as it is commonly understood depends on well-defined goals and fully developed understanding of the structure of the situation in which we are to be rational. It is easy to demonstrate rationality in solving an equation or devising a solution to an engineering problem, and it is possible to demonstrate irrationality in a wide range of situations. But it is very difficult to say what is rational behavior when goals are only partially specified, when few alternatives are known, and when the relationships between alternatives, goals, and the environment are only partially known. Since this is the case in most strategic decisions, we must conclude that we really do not know what constitutes rational behavior in strategic planning and decision-making.

Our purpose here is to point out that rationality in decision-making is not a simple concept. It is something that we will have to pay attention to in assessing the role of analysis in policy level decision processes. After we have discussed current concepts of systems analysis in this chapter and the impact of human behavior on organizational decision processes in the next, we will return to this question of rationality in a broader context.

E. Systems Analysis

There is no really satisfying definition of systems analysis. We use the term here to refer to explicit analysis of specific policy level decisions although "policy analysis" would be more descriptive. "Systems analysis" is widely used and understood in the defense community, however, and we will stick to that name. Military systems analysis as an identifiable activity arose during the nineteen-fifties as the result of attempts to apply operations research methodology to increasingly higher level problems of national defense. As a result it has much in common with operations research; but it also has its differences. Depending on one's view of the operations research method, it is possible to draw endless distinctions between operations research and systems analysis, but it is not really worth going into all the subtle distinctions. Basically, the differences are of emphasis rather than substance. Probably the most useful distinction between the two is that operations research usually is concerned with operational level (management control) problems that are better

understood structurally and can be made quantitative more readily, while systems analysis is concerned with policy level (strategic planning) problems that cannot so readily be made quantitative and where the structure of the problems is not so well in hand. The mathematical techniques that have proven useful in economics and management are therefore more consistently useful in operations research than in systems analysis, and operations research has tended to become more closely identified with those techniques -- especially in the academic world.

Alain Enthoven [16] has defined systems analysis as

> a cycle of definition of objectives, design of alternative systems to achieve those objectives, evaluation of the alternatives in terms of their effectiveness and costs, a questioning of the objectives and a questioning of the other assumptions underlying the analysis, the opening of new alternatives, the establishment of new objectives, etc.

This is considerably different from the usual definition of operations research and from the popular image of systems analysis as "decision by computers". In a similar vein, Hitch and McKean [17] in The Economics of Defense in the Nuclear Age (which is the nearest there is to a systems analysis textbook) say:

> It cannot be stated too frequently or emphasized enough that economic choice is a way of looking at problems and does not necessarily depend upon the use of any analytic aids or computational devices.

In short, systems analysis is a way of looking at problems at the policy level and organizing the information so gained in order to aid the decision-maker in making better decisions than would be possible otherwise.

If it is not computers or sophisticated mathematical techniques, if systems analysis is a particular way of looking at problems, what is it that distinguishes it from the way people usually deal with policy level decisions? There seem to be four particularly important characteristics that distinguish systems analysis:

1. Emphasis on understanding the structure behind the decision.

2. Emphasis on explicitness of the analysis.

3. Emphasis on the recognition and treatment of uncertainty.

4. Emphasis on goal directed rather than problem directed action.

Each of these four emphases represents a considerable departure from the processes by which most organizations develop their policy. In Chapter IV we will explore these differences in detail and how the introduction of systems analysis in an organization's decision process can have significant impact on the life of the organization.

To quote again from Enthoven [18]:

> National security policy decisions are based on the interaction of values, on the one hand, and the costs and effectiveness of military forces and weapon systems on the other.
>
> For most of these questions a mix of calculations, intuition and experience is required. One of the biggest challenges facing us today is how to find ways of blending these factors better in those areas in which unaided calculation is weakest.

Systems analysis is nothing more than the current state of the art in combining judgment and calculation to make strategic decisions in a way that results in effective strategic planning for an organization, whether it is the Defense Department or a large corporation or a hospital. So far it is still an art. There have been a number of "how-to-do-it" rules-of-thumb set forth, but these serve for the most part to remind experienced practitioners of important considerations. No one knows very well how to teach someone to do systems analysis, except by example. The principles of operations research and economics are useful mainly for the insights they give into understanding complex problems of resource allocation and operational efficiency, although occasionally the techniques themselves prove directly applicable.

How does systems analysis go about the blending of judgment and analysis? In addition to the four emphases listed above, there are five characteristics of systems analysis that are central to the method:

1. Use of proximate goals

2. Use of partial, ad hoc models

3. Treating judgments as variables rather than givens

4. Search for improved systems in addition to evaluation of existing systems

5. Iteration of the process

To complete the discussion of systems analysis, we will discuss each of the four characteristic emphases and the five characteristics of the method of systems analysis.

If one had to single out as most important to the success of systems analysis one of these nine characteristics we have listed, it would have to be the first: the emphasis on understanding the structure underlying the decisions. From this a number of benefits arise. First, it provides the decision-maker with added insight into the problem. At the policy level of an organization, analysis alone cannot produce a decision; the decision is made by the man responsible, and analysis is no more than an input to his decision process. By improving his insight, analysis enables the decision-maker to integrate more and better the many factors that he must consider, and thereby reduces the chance that he will misinterpret the decision he has to make. In systems analysis the emphasis is often on making sure we have asked the right question; this is just another way of saying that we have

found the essential structure underlying the decision. Secondly, improved understanding of the structure permits better evaluation of alternative systems by showing which analytical techniques are applicable where. Finally, it improves our chances for finding improved alternatives by giving insight into the relationships between goals and systems. Any one of these three benefits can be of immense value in dealing with the decisions that arise in strategic planning; so great is our lack of understanding of most great policy issues that such basic analysis can have considerable payoff.

A word of warning should be inserted after such great claims. The relationships that are relevant to most policy issues are so numerous and complex that we can never expect to understand fully the structure behind the decisions in most policy areas. This means that the structure we speak of is really the perceived structure at a point in time. Further, decisions are weighed in the context of the structure as it is perceived. It is not surprising then, when a "breakthrough" is made to a new perception of the structure, that old solutions seem clearly inferior to new ones that are made possible by the new context. Whether they are <u>in fact</u> inferior depends as much on the quality of the structure as on the quality of the system within that structure. Although we are in a situation where two "wrongs" could be better than two "rights", we clearly are less likely to go astray if we trust to our structure as we perceive it. Further, our structure is an integral part of our value system, and only if we could be shown to be non-Pareto-optimal in terms of goals <u>and</u> structure could a strategic decision be demonstrably inferior.

The emphasis on explicitness in systems analysis shows up in many ways: in stating goals, in stating assumptions, in identifying uncertainties. By being explicit on such matters in the course of the analysis, we can better assess where the strong and weak points of the analysis lie and are in a better position to improve it. Similarly, we are better able to see how to improve on the available alternatives. The emphasis on the recognition and treatment of uncertainty really derives from the explicitness of analysis. Uncertainty about technical and operational parameters and about the structure of the problem abounds in strategic decision problems. Facing these uncertainties and trying to devise alternatives that are relatively insensitive to them rather than working only on the basis of best estimates makes the systems analysis approach qualitatively different from the way decision problems are usually treated.

The emphasis on goal directed action rather than problem directed action is also a major difference between the systems analysis approach and other approaches. This is, of course, consonant with the rational ideal of starting with goals and deriving the appropriate action, but it is not in agreement with the way most organizations function. Pounds [19] found that most of an executive's time is spent dealing with problems presented by other people rather than on the goals they think are most important. There are some significant organizational reasons for this phenomenon which we will get into later, but it is also true that it is much easier to devise an answer to a given problem than to decide what the problems should be in addition to devising the answers.

At the policy level of an organization, goals are likely to be very general and somewhat imprecise. As a result, they are not operational; i.e., they do not serve to indicate uniquely which alternatives are better than others. In defense, for example, we have the goal of deterring general war; in a firm we have the goal of profit. Deterrence is not a particularly precise concept; it has many dimensions, and many of them are subjective and qualitative. Similarly, profit is a very vague concept; some of its dimensions are short-term earnings, dividends, retained earnings, and future profitability. In order to deal with such imprecise goals, systems analysis uses proximate goals that serve as indicators of some aspect of the more general goals. For example, in defense we use the proximate indicators of damage limiting, assured destruction, flexible response capability, alliance structure and others. In the firm, common proximate goals are market share, rate of return, extent of diversification, and relationships with suppliers. Some of these can be made relatively precise and quantitative while others are only qualitative indicators, but each is a more limited and therefore more readily assessed indicator of performance than the overall goals. Some of these proximate goals are also proxy goals; market share, for example, is not so much a direct goal as a proxy for long-term profit.

One reason for multiple goals in strategic planning is that we do in fact have several largely unrelated goals. Another reason is that our understanding of the structure of the problem is so limited

that we are unable to see how subgoals relate to one another well enough to combine them into a more comprehensive single goal. Similarly, we are unable to describe fully the relationships between the alternatives and the goals. In order to cope with this limitation, systems analysis makes use of judicious factoring of the problem into related sub-problems. By identifying cohesive problem areas whose internal relationships can be fairly well understood and which have relatively few links to other problem areas, we can reduce the intellectual capacity required to deal with the larger problem. In effect, each sub-problem is solved independently, and the solutions then are combined to produce the total solution. Combining the solutions will, of course, necessitate going back and adjusting the original solutions to account for the interactions among the sub-problem areas, but several repetitions of this process are feasible while simultaneous treatment of all the relationships usually is not.

The effect of such an approach is the use of whatever models are appropriate for each individual sub-problem. And the definition of the sub-areas will depend on the particular characteristics of the decision to be made at a given time. The result is an approach to analysis that uses a number of ad hoc models to represent the aspects of the alternatives and the environment that are relevant to the particular decision to be made. This is in contrast to some approaches to analysis that attempt to model completely the operation for which decisions are to be made. The goal of an all-purpose model is laudable, but unfortunately leads to excessive simplification, inflexibility, and

lack of pertinence. In strategic planning, the good analysis is the one that makes a difference in the decisions that are made, and it is difficult to relate comprehensive models to the specific issues on which decisions are made.

This approach leads to the inevitable phenomenon of suboptimization. In operations research literature, this has acquired a pejorative connotation, while in the systems analysis literature, clever suboptimization is much praised. Lindblom's criticism of the rational-comprehensive "root" method of decision [20] rests primarily on attempts to avoid suboptimization: "futile attempts at superhuman comprehensiveness". The fact is, of course, that our limited understanding of the structure of a decision and our limited intellectual and computational capabilities make suboptimization the most reasonable way to approach the analysis for such decisions. It goes without saying that unnecessary suboptimization should be avoided; it is not something we do to make life easier, but something we do because we have no better way of dealing with extremely complex situations. Part of defining the problem or asking the right question or improving our understanding of the structure of a decision is finding the natural breaks that produce a factoring of the problem into sub-problems whose individual optimizations produce a result readily related to a more global optimization.

Most approaches to analysis for decision-making take into account the distinction between value judgments and judgments on questions of fact. It is not always possible to separate the two -- as in the case

of the "cost" of a stockout in inventory policy decisions -- and although systems analysis tries to be explicit in distinguishing the two, this is not unique. Rather, it is the treatment of judgments as variables, as parts of the problem, rather than as inputs to the analysis that systems analysis differs from most other approaches to analysis. Most rational approaches to decision start with judgments about values and of factual questions and proceed deductively to the "solution"; systems analysis emphasizes the use of analysis to help the decision-maker to make better judgments.

Even when it is possible to structure the decision sufficiently well to isolate the judgments required, it is difficult for the decision-maker to specify them in the abstract, apart from the context of the decision. This is because so many factors impinge on his judgment that he can place little confidence in judgments made apart from real consequences. The utility function or indifference map of microeconomics and the preference function used by the Bayesian analysts are useful concepts, but it simply is not practicable to ask a decision-maker to specify and then use such devices for capsulating his judgments in most strategic decision problems. Further, the time required for the decision-maker to construct a utility function would be unrealistic because of the large number of tradeoff possibilities he would have to consider. To do this carefully would require more time than it is possible to give and would necessarily require many unnecessary tradeoffs since only those in the vicinity of the final

solution need be carefully thought out. And if the tradeoff judgments required to use such a device were not made carefully, there would be little confidence in the result of the analysis.

Lindblom has pointed out that in real life, values and alternatives are intertwined; that we can choose among values only in the context of some alternatives that differ along those value dimensions. Systems analysis attempts to help the decision-maker make better and more relevant judgments. By improving the structure of the decision, analysis can help clarify just what judgments are relevant. By tracing out their implications in terms of the alternative that would be chosen and the performance along the various goals, analysis can provide the decision-maker with information that helps him converge on better and better judgments; i.e., judgments that reflect better and better his assessment of the problem.

(A common conception of the procedure for analyzing decision problems is that assumptions, goals, and alternatives and their consequences are set out making full use of the appropriate judgments; rational deduction then leads to the solution -- the decision. Changing assumptions or goals or relationships after the implications are seen is too often regarded as "cheating" -- as allowing caprice or bias to creep in. Systems analysis recognizes that this simply reflects (or should) a refinement of the original judgments based on improved information made available to the decision-maker.)

Next to understanding the structure of decisions, the biggest contributor to the success of systems analysis is probably that it is more synthesis than analysis. Just as analysis is used to elicit better judgments from the decision-maker than he would be able to supply unaided, analysis also helps to suggest improved alternatives. It seems to be especially true at the policy level that few reasonably attractive alternatives are available. It is certainly not true that all or even most of the relevant alternatives are known before the analysis begins. In fact, many policy level decisions arise only because a new alternative is presented; in the absence of new alternatives, only changed goals or evaluation techniques would provide the impetus for strategic planning activity. Thus we can trace much of the growing attention to policy level analysis to the rapid rate at which new alternatives are being introduced as a result of technology.

Analysis can contribute much to decision-making by tracing out the implications of known alternatives in terms of specified goals. However, just as we have pointed out that systems analysis treats goals as part of the problem rather than a priori givens, it also uses the evaluation process to suggest improved alternatives. Some examples of how this is achieved are the use of breakeven analyses, hedging against uncertainties, looking for dominant alternatives, and generally trying to see where changes in existing alternatives would achieve a clear improvement. Since so much of finding a new and better

alternative is simply imagination and creativity, almost by definition we know little about how it is done. Wildavsky [21] has caught this and many other attributes of systems analysis:

> The good systems analyst is a ... wise man with overtones of wise guy. His forte is creativity. Although he sometimes relates means to ends and fits ends to match means, he ordinarily eschews such pat processes. Instead, he imaginatively relates elements into new systems that create their own means and ends. He plays new objectives continuously against cost elements until a creative synthesis has been achieved. He looks down upon those who say that they take objectives as given, knowing full well that the apparent solidity of the objective will dissipate during analysis and that, anyway, most people do not know what they want compared to what they can get. Since no one knows how to give instructions for creativity and daring and nerve, it is not surprising that no one can tell you what systems analysis is or how it should be practiced.

The last characteristic of systems analysis listed was the iteration of the analytic process. The rational-comprehensive ideal is so often presented as a sequential process that "equal time" should be given to the opposing view. To someone who has never learned to analyze his decisions systematically, the linear progression from setting goals to listing alternatives to comparing their costs and benefits to choice is a worthwhile lesson. But anyone with any sophistication at all in analysis for decision-making will realize that this is too drastic a simplification. Systems analysis is a cut-and-try process since we are addressing problems that we do not understand very well. To quote Enthoven [22] once more, systems analysis is best described as

> a continuing dialogue between the policy-maker and the systems analyst, in which the policy-maker asks for alternative solutions to his problems, makes decisions to exclude some, and makes value judgments and policy decisions, while the analyst attempts to clarify the conceptual framework in which decisions must be made, to define alternative possible objectives and criteria, and to explore in as clear terms as possible (and quantitatively) the cost and effectiveness of alternative courses of action.

It should be clear by now that systems analysis is not a technique for supplying decisions. Rather it is a process for identifying information for decisions and developing and displaying the information for the decision-maker. The emphasis is on providing information that will help the decision-maker better understand the decision he faces. As an information processing tool rather than a decision rule, systems analysis requires intimate involvement of the decision-maker in the analytic process. As Enthoven pointed out, it is a continuing dialogue rather than a one-shot service. In the simplest terms, systems analysis gives the decision-maker the opportunity to ask "What if..." and to be asked "What about..." The decision-maker and the analyst are dependent on one another; the analyst needs the decision-maker to keep his analysis relevant and useful, while the decision-maker needs the analyst to keep him informed of the consequences and implications of possible decisions.

So far we have managed to discuss systems analysis with only cursory reference to the use of computers and quantitative techniques that has attracted so much attention. As has been pointed out, these

techniques are not central to the systems analysis process, but they do play an important role in systems analysis for several reasons. One not insignificant reason is that the people who define systems analysis as what they do, mostly have a considerable background in economics and operations research; it is not surprising then that the jargon of systems analysis is somewhat technical. More important, some of the sophisticated quantitative techniques actually are useful in some policy level decisions. The use of a linear programming model to give least-cost systems for meeting airlift/sealift requirements is an example. Further, the pedagogical value of the techniques is high. As idealizations of resource allocation decisions and operational optimization decisions, some added insight into the character of decisions can be gained from study of these techniques. Another benefit of such exposure is that certain concepts useful in discussing decisions have their origin in various mathematical idealizations; the concepts of shadow price and equated marginal costs for efficiency and a familiarity with the behavior of dynamic systems are all useful in the systems analysis process even though the underlying idealizations may not be directly applicable.

The attention given to the quantitative aspects of systems analysis by the press and by the armed services is not due so much to its use of sophisticated mathematical techniques but to the fact that it includes quantitative considerations at all. Decision-making at the policy level of organizations has traditionally been a qualitative matter; the profusion and successes of liberal arts majors and lawyers

in the top management jobs of government and industry are evidence of this. Budgets, of course, have always been quantitative matters, but the criteria for allocating budgets usually have not. By introducing quantitative measures of effectiveness and explicitly addressing the tradeoffs of a budget dollar among various alternatives, systems analysis has introduced a foreign element into the policy level decision process. The fact that these considerations were always there implicitly does not alter the fact that this is a significant change in the decision process. In Chapter IV we will explore in more detail the reactions to systems analysis that result from this explicitness.

Systems analysis is not without its dangers. More traditional processes for making strategic decisions often produce inefficient and ineffective decisions by failing to be explicit about objectives and the tradeoffs among them that are implicit in any given decision and by failing to delve deeply into the structure underlying the decisions. One major danger of systems analysis (the one that is most often cited by its critics) is that those issues that cannot be represented quantitatively or cannot be fitted into the perceived structure in any very satisfactory way will be overlooked or de-emphasized in comparison to those that can be explicitly structured and quantitatively represented. This can be called the Gresham's law of analysis: the quantitative tends to drive out the unquantifiable. By definition, the good systems analyst achieves a balance between these two types of factors in his analysis, but it is a trap which even the most experienced must be careful to avoid. Since we tend to

reify our images of the world -- to mold the systems we deal with into patterns that agree with our conceptions -- this may not be quite so serious a flaw in the long run as it seems on the surface; after all, we have been making decisions on the basis of what we perceive for a long time now. For example, Keynesian economics clearly has its limitations; but as we reshape our economic institutions with a relatively Keynesian view of things, Keynesian economics becomes less and less limited in dealing with our economic problems.

A second major danger of systems analysis is the analyst enamored with analysis. All professionals have the natural tendency to appreciate elegant and artful application of their craft, and concern with the technique per se is a very proper concern. But the practice of systems analysis, like that of medicine, is judged by the help it gives the customer and not by the satisfaction it gives the practitioner. A philosopher once called this phenomenon the "Rule of the Tool", which he stated in its simplest form like this: "If you give a small boy a hammer it will just happen that everything he sees needs pounding." With the many specialists that contribute to a systems analysis study, care must be exercised to assure that tools are chosen for their usefulness rather than for their availability.

Still another danger of systems analysis -- and this is probably the most serious danger -- is that creativity and dissent within the organization will be stifled. On the surface this is a contradiction of the point just made that systems analysis encourages and derives

its success from creativity. The danger is that systems analysis tends to function as though there were some underlying, implicit utility function for the organization (even though it rarely attempts to formulate it explicitly) and to provide the information that will enable the decision-maker to converge on a decision consonant with that underlying set of values. When systems analysis is imposed as the rules of procedure on the strategic planning activities of the organization, new alternatives, new structures, new emphases on goals all must go up against the explicit structure that has been evolved through analyses of strategic decisions. The painstaking process of gaining acceptance through a highly explicit analytic process can have the effect of screening out many good ideas because no one is sufficiently committed to them to pursue the process. We will have more to say about this and related topics in Chapter IV.

F. Program Budgeting

Program budgeting is really a very simple concept. It is basically only a different way of presenting the allocation of an organizational budget. In line item budgeting, expenditures are displayed by item of expenditure (personnel, desks, widgets, ...), while program budgeting displays expenditures by objective (education, welfare assistance, national security, ...). Any budget display calls attention to the relative amounts allocated to each budgetary category and suggests tradeoffs among the categories. In the line item display it is natural

to ask whether funds might usefully be shifted from desks to widgets, while in a program budget display tradeoffs among objectives such as vocational training and pre-school education programs are suggested.

For purposes of strategic planning, the program budget is clearly more relevant. The line item budget is more appropriate for the operational control and management control activities. However, no one categorization of a program budget will be appropriate for an organization. There are many ways to slice the budget categories and each will call attention to certain tradeoffs while submerging others. No one is best for all purposes. For example, there was recently considerable debate over whether basic research activities should be categorized by discipline or by type of institution in the federal budgeting process. The answer lay in realizing that we have objectives for both disciplines and institutions, and the appropriate categorization depends on the particular decisions being made and issues being addressed at a given time rather than in artificially imposing one or the other as "proper".

A budget is many things: it is a plan, it is a decision, it is a model, and it is a goal. Budget information for use in operational control and management control should not be expected to be necessarily relevant to strategic planning. For strategic planning, the budget should be formulated along with organizational goals and the program budget categories should conform as closely as possible with broad organizational goals. Those responsible for making the strategic

decisions that go into the budget allocation process should take a broad view of these decisions. By reviewing broad program alternatives in the context of an objectives-oriented program budget, the strategic decisions that must be made and the long range planning function can be integrated effectively.

Although program budgeting and systems analysis have been linked in Defense Department decision-making under McNamara and more recently in the Programming-Planning-Budgeting System of the Bureau of the Budget, the two are not necessary to one another. A program budget could be adopted without using systems analysis to help make the decisions on resource allocation, and vice versa. However, in order to institute program budgeting into the organizational budgeting process, the objectives of the organization must be set out explicitly and their interactions explored. To capitalize on this effort, it is natural to use explicit objectives-oriented analysis of alternative objectives and alternative systems in order to make the decisions that determine the actual allocation of resources among the program budget categories. Conversely, once decisions are being made with the aid of systems analysis, it is natural to adopt a program budget breakout -- at least for purposes of analysis. In short, systems analysis and program budgets are natural partners in the strategic planning and decision-making process. Adopting only one of these decision-making aids without the other would not be realizing the full potential of either.

In performing systems analysis decisions are examined over the relevant time span of the alternatives. This suggests to a multi-year view of decisions and the consequent use of multi-year programs as a planning tool in addition to the program budget for the current fiscal year. This helps assure that the future implications of current decisions are considered and that the current budget decisions will not produce too severe strains in future years.

We have stressed the application of systems analysis and program budgeting in the strategic planning process. They are also applicable to the management control function as well. Program evaluation in terms of the objectives of the programs and the resources allocated to them requires essentially the same techniques as deciding in advance which resource allocations would most effectively realize the objectives of the program. Effective evaluation of program effectiveness and efficiency in turn provides a firmer basis for systems analysis in the strategic planning function.

CHAPTER III

Behavioral Theories of Decision-making

The preceeding chapter was occasionally a little strained by ommission of the relationships between systems analysis and the patterns and motivations of human behavior in organizations. In this chapter, we will focus on these human determinants of organizational performance and in particular on the theories that have been developed along those lines. Once again, however, we will be somewhat strained from time to time by omitting the relationships between these factors and the considerations developed in the systems analysis chapter. This is done partly to emphasize the schism between behavioral theories and rational theories of organizations, but also in order to have both sides in hand before exploring how the two types of factors interact. That interaction will be developed in Chapter IV.

A. Organizational Theories

The heading of organizational theories covers an extremely wide ground. March and Simon took 210 pages to summarize the literature on organizations, and the result was extremely concise; many pages more could be written on each of the concepts they identify without

exhausting relevant topics. No attempt is made here to summarize the literature on organizations; the reader who wants to study that directly is referred to the remarkably concise Organizations by March and Simon [23]. Rather, we will skim the surface of organizational theory for the benefit of those who may not be at all familiar with it and then touch on those points that appear most relevant to the use of systems analysis in the strategic planning process of an organization.

March and Simon point out that much of what we know about organizational behavior follows directly from experience and common sense. Formal theories of organizations have been based on these grounds or on abstract postulates. "The literature contains many assertions, but little evidence to determine -- by the usual scientific standards of public testability and reproducibility -- whether these assertions really hold up in the world of fact" [24]. This is expecially true of the hierarchical top of organizations. Top management in the business world has traditionally not been open for study by social scientists, and what we know about this rests largely on the impressions of experienced executives. Similarly, in government the "inside" decision processes are not open for explicit study and political science has had to rely on the recollections of participants for insights. This has made it very difficult to evolve useful principles about organizational behavior. The intellectual capacities and predilections appropriate to a participant in the policy level decision process of an organization differ from those appropriate to the objective study of the process. To exaggerate, the result is

theories by those with limited appreciation of what the subject is really like and the relatively unstructured "wisdom" of those who have learned to act in the process but who are so caught up in it that they can step outside it only to a limited extent.

Cyert and March [25] categorize organization theory into three major branches: sociological theories, social psychological theories, and administrative theories. If we confine our attention to those aspects of organizations that pertain to the organization as an operation rather than a social system, these three categories comprehend the most relevant literature.

Sociological theories are described as those views of the organization as a mobilization of human efforts to achieve given organizational goals. The emphasis here is on rational division of labor and specialization of functions in order to carry out the operations of the organization efficiently. Weber's theory of bureaucracy is probably the most famous work in this category. There was originally a tendency toward depersonalization as a reaction to the earlier amorphous and personal character of organizations. Objectivity, rationality, and predictability were stressed over arbitrary rules and personal whim. Hall [26] has cited six dimensions of bureaucracy:

1. Division of labor based on functional specialization.
2. Well-defined hierarchy of authority.

3. Rules covering the rights and duties of incumbents.

4. Impersonality of interpersonal relations.

5. Systematic procedures for dealing with work situations.

6. Promotion and selection based on technical competence.

The organization is viewed as a collection of operations to be subdivided and related to one another in a way that will make for minimization of caprice and maximization of efficiency. Deviations from the mechanistic ideal were treated as pathologies to be corrected. More recently, this body of literature has addressed different types of bureaucracies, the life-cycle of bureaucracies, and the classification of individual personality types in bureaucracy.

The social psychological approaches to organizations have been much more limited. Rather than address the organization as a whole, these theories have focused on individual tasks within the organization and examined the effect of external variables on the efficiency of the operation. The methodology is to select a small number of independent variables and to examine experimentally the effect of these variables on the efficiency of the operation by using tests of statistical significance. The Hawthorne experiment to determine the effect of improved working conditions on worker productivity is probably the best known example of this type. The RAND studies of early-warning radar system operations is another well-known example. Despite its

obvious connection to the scientific management movement, this approach has led to considerable work on industrial psychology, worker motivation, and morale. The emphasis in all this work is on relatively well-defined and repetitive situations, in part because of the available statistical methodology and in part because these were seen as the significant problems of the organization.

Administrative theories center on such issues as centralization and decentralization, side payments and the decision to participate, and individual perceptions and expectations. By its focus on decision-making rather than on the arrangement and efficiency of operational tasks, this branch of organization theory is more relevant to the strategic planning process than the other two branches. Some of this literature is concerned with the transfer payments within the organization that bring members into coalitions and maintain them. The inducements-contributions theory of the decision to participate in an organizational coalition is an example of this focus. More recently there has been an emphasis on the way in which decisions are made in organizations. The focus here is on the relationships between individual and organizational goals, role influence, and perceptions and expectations. Much of this is based on cognitive psychology and the psychology of problem-solving thought processes.

Cyert and March [27] conclude that organizational theories provide a very limited basis for a theory of the firm:

> The sociological and social psychological approaches have emphasized questions that are only marginally relevant to either the objectives of conventional theories of the firm or the objective of predicting individual behavior. The decision-making approach has developed a substantial theory of decision-making processes in an organizational context, but has not applied the theory to the specific environmental conditions in which the business firm operates nor applied the theory in detail to the particular decision variables that characterize the firm's operation.

B. Recent Developments

Recently, two major approaches to the behavioral side of organizations appear to have evolved. The first is an outgrowth of the social psychological and sociological theories described above. It is something of a reaction to the mechanistic view of organization brought about by bureaucracy. This approach focuses on "the inescapable tension between individual and organizational goals." It is oriented around the conflict of "the individual's needs, motives, goals, and growth versus the organization's goals and rights" [28]. This approach focuses on the interpersonal environment within the organization rather than on the impact of behavioral considerations on specific decisions. The organization's goals are taken as given, and the search is for a process of interpersonal interaction that will minimize interpersonal conflict in meeting those goals.

The second recent approach attempts to integrate economic and cognitive psychological factors in looking at the decision-making processes in organizations. Cyert and March in The Behavioral Theory

of the firm [29] have concentrated on industrial organizations and Lindblom [30] has addressed similar topics in government decision-making. These three authors together with Simon and his work on the concept of the organizational goal [31] form the basis for a view of organizational decision-making that appears to be more relevant to the role of analysis in the strategic planning process than any of the other approaches.

Cyert and March view the organization as a coalition. Entrepreneurial and consensual goal mechanisms are rejected in favor of goals formed through bargaining in the formation of coalitions. This bargaining among individuals and coalitions in the form of making side payments to achieve goals represents the "central process of goal specification" in their theory. They assume that there is hierarchical asymmetry and that interpersonal joint preference orders are not formulated. The result is goals that are imperfectly rationalized, that are expres ed as aspiration-level constraints, and/or that are nonoperational. Limited time and cognitive abilities constrain the organization and the variability of the environment. In their view, conflict is never fully resolved; attention to goals sequentially and in separate parts of the organization together with organizational slack permit the necessary decisions to be made in spite of inconsistent goals.

Cyert and March reject the classical theory of expectations which postulates continuous competition among all alternatives for all resources, search for alternatives as one of the many competitors

for resources, and considerable calculation to evaluate and compare alternatives. Instead, they postulate that resource allocation will be done with rough screening, refined attention only to local problems, and early commitment to an alternative via a mixture of personal, suborganizational, and organizational goals. Search in their theory is motivated by problems and is characterized by early commitment to alternatives followed by subsequent intensification of search to accommodate suborganizational and personal goals through bargaining over side payments. They postulate that computations will be simple and involve as few dimensions as possible to avoid cognitive limitations in dealing with multiple criterion dimensions. Standard operating procedures will be evolved for repetitive situations and rules of thumb for roughly similar non-repetitive situations in order to minimize computation and to avoid uncertainty.

Their theory is summarized in four propositions:

1. Quasi-resolution of conflict:
    a. Goals as independent constraints
    b. Local rationality - limited goals and limited problems
    c. Aspiration-level decision rules
    d. Sequential attention to goals

2. Uncertainty avoidance
    a. Feedback - react decision-making
    b. Negotiation with the environment

3. Problemistic search

   a. Motivated, problem-oriented search
   b. Search "near" the problem symptoms and "near" the old system
   c. Search in organizationally vulnerable areas
   d. Biased search

4. Organizational learning

   a. Adaptation of goals
   b. Adaptation of attention rules
   c. Adaptation of search rules.

Lindblom focuses more explicitly on the decision-making process and contrasts two fundamentally different approaches to decision-making. In the evolution of his ideas over a period of several years, each of these approaches has assumed several descriptive names. The names he has used for the "Root" and "Branch" methods are:

Root:	Rational - comprehensive
	Synoptic (Rationalism)
	Central Coordinated Decision-making
Branch:	Successive Limited Comparisons
	Disjointed Incrementalism
	Mutual Accommodation of Partisans

He characterizes the root method by the listing of values (goals) and alternatives, systematic comparison of the alternatives against the values, and choice of the alternative that maximizes the values.

Branch, on the other hand, is characterized by setting a simple goal with or without explicit thought, listing a few alternatives that suggest themselves, comparison of the alternatives against past experience with similar alternatives, and choice simultaneously among values and alternatives. Lindblom contends that the root method assumes more information and intellectual capacity than human decision-makers possess and that it is unrealistic in terms of the time and resources available for analysis. He compalins that this method tries to start from fundamentals anew each time and uses experience of the past only as it has been embodied in explicit theories. The branch method by contrast builds from the current situation by small cut-and-try steps. Root is what we formalize, idealize, and teach and preach as the way decisions should be made; branch is what decision-makers in fact practice.

Lindblom distinguishes the two methods on five dimensions:

1. Separation of values and alternatives (means and ends)
2. Means - ends analysis applicability
3. Test for goodness of a decision
4. Comprehensiveness of analysis
5. Reliance on theory

Evaluation and search for alternatives are intertwined because identification of objectives is difficult and because agreement on objectives among members of the organization is not feasible. Values

are considered through preferences among particular alternatives and on the margin by tradeoffs that establish one alternative as preferable to another. Margainal objectives cannot be stated except in terms of particular alternatives. Which is more rational, Lindblom asks: the impossible and irrelevant specification of objectives prior to analysis or the possible and relevant intertwining of objectives with analysis?

It follows from this view that strict ends-means analysis is not possible in practice. How then can we know whether a decision is a "good" decision? Lindblom notes that without agreement on objectives there is no standard of correctness. The only practicable test is agreement on the chosen alternative itself, and this is the test of the branch method. To seek agreement on objectives would "accomplish nothing and create quite unnecessary controversy." Objectives "have no ultimate validity other than they are agreed upon." "In an important sense, therefore, it is not irrational for an administrator to defend a policy as good without being able to specify what it is good for" [32].

Lindblom argues that the comprehensiveness of the analysis assumed in the root method is not feasible in practice and that the manager cannot even comprehend one policy alternative fully. In order to cope with the decision-making function we must simplify, and the branch method simplifies in two ways. Only a few alternatives are considered with only a few consequences for each, and only marginal

change from the current situation is considered. He contends that even if alternatives and consequences and values are considered at random, we may get more intelligent choice than with "futile attempts to achieve a comprehensiveness beyond human capacity" [33]. Since almost every interest has its watchdog in the organization, the branch method often assures a more comprehensive regard for values than attempts at intellectual comprehensiveness. It also encourages decentralization and does not tend to suppress dissent. In Lindblom's view, the simplification of the root method is "accidental, unsystematic, and not defensible" while in the branch method, simplification is "deliberate, systematic, and defensible" [34].

Lindblom characterizes the root method as relying completely on theory and being inapplicable where no theory has been developed, while in the branch method no theory is needed. He sees the iteration of choice and comparison in successive increments as the distinctive element of the branch method. Lindblom concludes that the branch method is not a failure of method for which administrators ought to apologize, but the only practicable way to approach real-world decision-making.

More limited in scope, but in the same spirit as Cyert and March and Lindblom, Simon has looked closely at the concept of the organizational goal [35]. He draws the important distinction between goals and motives from which goals are evolved. He concludes that it is more reasonable to speak of a set of goals or constraints than a single

single goal and introduces the idea of feasible and Pareto-optimal sets of alternatives.* By using a linear programming formulation, he notes the parallel between goals and constraints. Goals may be used in two ways: to generate alternatives (synthesis) and to test alternatives (evaluation). Which "goals" we choose as goals (alternatives generators) and which we choose as constraints (tests) may influence which feasible alternative is discovered and subsequently selected.

If we use the term "goal" to imply sets of constraints or aspiration levels, then organizations probably do have goals in the sense of widely-shared conceptions of these constraints. But if we use goals to mean alternative generators, then there probably is much less agreement among individuals and suborganizations. Role behavior depends on means-ends premises as well as goal premises, and this is a source of personal and professional differences in decision-making style. Since the role a person occupies in the organization heavily influences the information received and the interpretations given to it, differences in perception, subgoal formation, and expectations derive from the position a person occupies in the organization as well as from his personal motives. "The discrepancies arise out of the cognitive inability of the decision-makers to deal with the entire problem as a set of simultaneous relations, each to be treated symetrically with the others."

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*Feasible alternatives are those that satisfy the constraints on the decision. Pareto-optimal alternatives are those that are not out-performed on all goal dimensions by any other alternative and therefore cannot be decided among except through value judgments.

In view of the hierarchical structure of most organizations, it is reasonable to refer to the organizational goals as the "test" and "generating" goals of those at the upper levels. Since one man's goal is another man's constraint, the subordinate employees will tailor their choices to satisfy the constraints established by higher echelons; they will not necessarily adopt the same generating goals as those at the higher echelons, so they will not necessarily act to optimize along the goal dimensions of their superiors. This can result in unresponsiveness of suborganizations to desires of those at higher levels and in apparent autonomous behavior by the suborganizations.

C. Psychological Theories

In Chapter II we discussed the aspects of decision as though there were a single decision-maker. In this chapter we are focusing on the fact that decisions are not made by an isolated decision-maker but within an organization. But parallel to this division is the distinction between economic and cognitive aspects of decision. The cognitive aspects include those aspects that pertain to human behavior while the economic aspects include those pertaining to the functions and objects by which the organization produces its output. We could have discussed the individual psychology of decision in Chapter II, but it seems to fit more naturally along with the organizational concepts.

There has been much work done on the psychology of problem-solving, but almost all of it is in the same vein as the social psychological organizational theories discussed above. The focus has been on the

influence of a few independent variables on the speed and accuracy of problem-solving in well-defined repetitive tasks in a laboratory environment. As a result, little of this work is applicable to strategic decision problems in an organization. The work of Festinger [36] and of Miller, Galanter, and Pribram [37] does, however, offer some insights into decision-making that seem applicable in the strategic planning context.

Festinger's theory of cognitive dissonance postulates a post-decision divergence in the relative attractiveness of the alternatives in favor of the one chosen. In the pre-decision period, the individual experiences conflict as a result of "mutually incompatible response tendencies" or, in other words, competing goals. In the post-decision period, he experiences dissonance as a result of the goal possibilities he has had to forego to achieve the goals associated with his chosen alternatives; in other words, he is aware of the opportunity costs he incurred in choosing the alternative he did. The interesting aspect of this theory of decision is the post-decision process. By focusing on the dissonance immediately after the decision, the decision-maker may exaggerate to the point of temporarily regretting his choice. This transient regret presumably disappears as the result of a search for information that will help reduce the dissonance.

Anticipation of dissonance before decision may influence the behavior of the decision-maker. In particular, the search for information prior to decision may be biased to avoid post-decision

dissonance, or if the decision-maker is confident of his ability to cope with dissonance he may seek information relatively objectively. Dissonance anticipation may also lead the decision-maker to adopt a minimax regret criterion rather than an expected benefit criterion; this could contribute to the hyper-conservative behavior of bureaucracies.

Miller, Galanter, and Pribram distinguish between Images and Plans in human thought processes and explore the relationships between the two. They define a Plan as any hierarchical process that can control the order in which a sequence of operations is to be performed; it is like a computer program and can be very detailed or very vague and flexible. The Image a person holds is all the accumulated knowledge he has about himself and the world, including values and facts -- organized by whatever concepts, images, or relations he has been able to evolve. Thinking is viewed as the manipulation of the Image by Plans and the development of actions that will help fulfill the Image.

Just as Simon noted the differences in thought processes due to differences in means-ends premises and differences in goal premises, this would be viewed here as differences in the Plans used and differences in the Images held. The decision-maker may have conflicting elements in his Image, and he may employ conflicting Plans; in the latter case he seems to be deliberately frustrating himself, but cannot discover why. Interpersonal conflict arises owing to difference

in Plans or Images. Plans as well as Images may be inappropriate in unfamiliar circumstances. "Reacting to an unfeasible Plan, we may change the Image to retain the Plan, change tactics and retain as much strategy as possible, or adopt a new strategy."

Some selected quotations from this work [38] will illustrate its applicability to provide insights into how we deal with unstructured decision problems:

> We can get caught looking for a solution that we would not be able to recognize if we had it.
>
> There are, fortunately many ways to compromise with reality, and people probably revise the Image as often as they give up the Plan. In ordinary affairs we usually muddle ahead, doing what is habitual and customary, being slightly puzzled when it sometimes fails to give the intended outcome, but not stopping to worry much about the failures because there are too many other things still to do. Then circumstances conspire against us and ... we may begin to suspect that we face a problem. But at first it is not clear what the problem is or what test would have to be satisfied by any solution.
>
> We search about, exploring a hunch, gambling that we might get a good idea if we spent some time on this or that, fiddling with a few examples, trying to imagine what is missing or what we could get rid of, but never being certain precisely what we are searching for. We are trying to construct a better Image of the situation. (We are) not sure there really is a problem, or, if there is, that any simple test for its solution can be found that will meet that test.

> The statement of the problem is revised repeatedly as we struggle with it, learn more about it, and build a richer, clearer Image of it.
>
> An ordinary person almost never approaches a problem systematically and exhaustively unless he has been specifically educated to do so. It is much more natural ... to visualize what is and what ought to be and to focus on the gap between them than to visualize some huge set of alternative possibilities through which he must search. In other words, the phenomenological aspects of problem-solving are more frequently connected with alternative Images than with alternative Plans.

The Image is very much like an implicit and crudely specified model of the world. It contains values, variables, and postulated relationships between them as does an explicit model. It is natural to assume that most people have implicit models of situations and use these models as the basis for decision-making. The major difference between this implicit model that is a part of the Image and an explicit model is that the implicit model is not recognized as a part of the analysis for the decision, but rather acts as a hidden generator of various types of judgments.

It is further postulated that individuals use meta-Plans to select which Plans to apply to various parts of the Image. Differences in personal and professional style in approaching decision-making can be attributed to differences in meta-Plans -- differences in the criteria we use for selecting the Plans for processing and adding to the information in the Image.

D. Concepts Relevant to Systems Analysis

We have touched on a wide variety of organizational and psychological factors, most of which are directly relevant to decision-making in the strategic planning function of an organization. Given the broad view of analysis we have adopted, these factors are also of direct relevance to the role of systems analysis in the organizational strategic decision process. Rather than condense these ideas any further into a summary, we will list here the more important ones for emphasis. These ideas fall very roughly into six categories: cognitive limitations, uncertainty, goals, role influence, conflict, and commitment.

1. Cognitive limitations
    a. Adaptive v. omniscient rationality
    b. Aspiration levels, satisficing, and optimizing
    c. Factoring and suboptimization
    d. Operational subgoals as proxies for non-operational goals
    e. Routinized behavior
    f. Heuristic search rules
    g. Sequential and decentralized attention to goals
    h. Incremental change

2. Uncertainty

   a. In structure v. in parameters

   b. Incremental view of the world and incremental change

   c. Negotiation with the environment

   d. Use of analogies, rules of thumb, and SOPs

   e. Absence of expectations v. lack of confidence in expectations

3. Goals

   a. Interaction of personal, suborganizational, and organizational goals

   b. Goals as constraints

   c. Motives v. goals

   d. Intertwining of values, goals with real alternatives

   e. Adaptation of goals from past experience

   f. Dissonance avoidance

   g. Alternative generators v. alternative testers

4. Role influence

   a. On expectations and perceptions

   b. On information received and its interpretation

   c. On the applicability of various Plans

   d. On the goals that are generators v. those that are tests

   e. On the side payments that can be made

5. Conflict

   a. Side payments and bargaining in coalitions

   b. Difference in Images, Plans, and meta-Plans

   c. Differences in alternative generators

   d. Biasing of information

6. Commitment

   a. Pressures to simplify search

   b. Pressures to make and honor side payments

This has been a very limited introduction to organizational and behavioral theories. Its purpose is to give some insights and to set the stage for the considerations to be addressed in the next chapter. There we will explore explicitly how we expect that systems analysis will be used by the participants in the strategic planning decision process of an organization and what its limitations will be. After examining two specific cases of how analysis was used in the Defense Department, we will return to see how these ideas are borne out.

CHAPTER IV

Systems Analysis and Organizational Behavior

In the last two chapters, we have discussed the structure of decision-making, with systems analysis as one particular approach to decision-making, and some of the organizational and individual psychological considerations that relate to the making of strategic planning decisions in an organization. In this chapter we will combine these three areas to explore what some of the uses and limitations of systems analysis might be, what the effects of using systems analysis might be on organizational behavior, and how systems analysis might be made an effective part of the strategic planning process.

It would be tempting at this point to suggest a theory of strategic decision-making in organizations based on a synthesis of systems analysis concepts and organizational theory concepts. Since there is an almost complete gap between the economic theory and the organizational theories, some kind of integration or synthesis of the two is clearly desirable. Unfortunately, however, we do not know enough about either to develop very satisfactory theories that combine the two. The most useful approach in this case is probably that taken

by March and Simon in Organizations -- to list a number of probable propositions and, as well as we can, their relationships to one another. Trying to organize such propositions into a concise coherent structure is very much similar to the problem of unstructured decision-making; it requires much cut-and-try muddling around before we finally make some sense out of it. This chapter is the result of trying to understand the implications of various concepts mentioned in the previous chapters, trying to relate these to more general principles, and then trying to organize these into some coherent sequence. In short, this is not a theory of systems analysis and organizational behavior here, but rather a collection of hypotheses that are the result of thinking about the role of analysis in organizational decision-making.

A. Analysis and Cognitive Limitations

Complexity that goes beyond the decision-maker's capability to comprehend produces conceptual uncertainties -- uncertainty about the structure underlying the decision and about the available alternatives -- that call into question the applicability of many of our ideas about rationality in decision-making. Cyert and March, Simon, and Lindblom have described the patterns of behavior that have evolved in practice for dealing with unstructured decisions and have pointed out the reasonableness of some of these procedures when decision-making is viewed in a wider context than the purely rational-economic. There are two major shortcomings of the work of these authors, however. The

first is that they have confused (or at least intermingled) phenomena deriving from cognitive limitations on the one hand and phenomena derived from interpersonal interaction in the organization on the other. The second shortcoming is their emphasis on satisficing behavior rather than on attempts at optimizing behavior.

It is certainly true that much of the theoretical work on decision-making has ignored the practicalities of the "real world" and that we need to know more about the way decisions are made in real organizations. But we also need to be careful to separate underlying characteristics from run-of-the-mill behavior that can be improved upon. There is little doubt that most organizations do little optimizing in their decision-making, but one almost gets the impression from these authors that attempts at optimization are futile and altogether inconsistent with the practicalities of organizational decision-making. It is true that in the face of complexity that overwhelms our cognitive capabilities, we cannot optimize in the global sense of the word; simplification is essential. But even though there always comes a point when we must say "good enough", we can <u>try</u> to reach the best decision we can within the limits of time, resources, and capabilities. The question is not whether we want to optimize or even whether we should try, but what ways of simplification will produce as good a decision as we are able to make within these limitations.

Cyert and March [39] and Simon [40] have put considerable emphasis on the concept of satisficing as an alternative to optimizing. This seems to be more useful in describing the behavior of organizations

over time than as a normative device for analysis at a point in time. It assumes the existence of an aspiration level for each goal dimension that is based on historical experience and does not address the question of how we would set such a level if we were to do so explicitly. It does not allow for organized and explicit search for the best alternative that can be found at a point in time. They are saying something stronger than that whatever the decision-maker tried to do, he was in retrospect only satisficing; the implication is that at a point in time he is content only to satisfice based on a fairly definite "good enough" aspiration level.

This view of satisficing is tied up with the concepts of sequential attention to goals and sequential search until a satisfactory alternative is found: The idea is that the decision-maker searches for alternatives until he finds one that meets or exceeds his aspiration levels which are based on historical experience, past performance, and past aspirations; if he cannot find such an alternative in a reasonable time, he will scale down his aspiration level until one is found. Even though we cannot optimize in the true sense of the word, there is another interpretation we can give to "satisficing" that is compatible with the spirit of optimizing within our limited time, resources, and cognitive capabilities. We pointed out in Chapter II that -- as Lindblom has said -- objectives and alternatives are intertwined and that objectives are formulated in the context of the available alternatives. One of the purposes of analysis is to explore the interaction of alternatives and objectives to permit the decision-maker

to assess better his objectives. This suggests that there is a cycle in analysis of defining objectives, devising and evaluating alternatives, redefining objectives, etc. This cycle is stopped at some point not because we suddenly find our true objectives or find the optimal alternative. Rather, the cycle is stopped because we make the judgment that further analysis is likely to have small returns in terms of better definition of objectives or the identification of improved alternatives, or has become too costly to be justified. In short, we view satisficing as the end result of explicit attempts to optimize through an iterative cycle rather than as a one-shot comparison of a few alternatives against a historically based aspiration. This is a retrospective satisficing rather than a prescriptive satisficing.

While systems analysis is distinguished from the usual approaches to decision-making in organizations by its attempts at explicit optimization, and although it draws on many of the rational-economic concepts, it is a far different thing than the rational-comprehensive idea that so many people tend to identify with systems analysis. To see just how different it is, the characteristics of systems analysis described in Chapter II can be compared with the "disjointed incrementalism" approach Lindblom cites as the rational way to act in the face of complexity and limited cognitive capability.

First, Lindblom notes the interdependence of alternatives and objectives (values are formulated in terms of preferences for available alternatives and are traded off at the margin); we have just noted that

systems analysis rejects the linear form of analysis from given objectives to an optimal alternative and instead explores the interaction of ends and means in order to help the decision-maker formulate his objectives for purposes of decision. Lindblom claims that we cannot be comprehensive, but that we can consider only a limited number of alternatives, effectiveness measures, and consequences; systems analysis uses proximate measures of effectiveness and ad hoc partial models. Lindblom claims that because we must simplify, we must move incrementally and by successive steps of ... ce and comparison; systems analysis emphasizes the iteration of analysis and decision to improve the relevance of the analysis to the decision and to improve the information base for decision.

If this juxtaposition of Lindblom's "muddling-through" method of disjointed incrementalism and the supposedly very rational systems analysis seems contradictory, then two points should be made for emphasis: (1) Lindblom has made a useful description of the impact of cognitive limitations and complexity on what kinds of approaches to analysis for decision-making make sense, and (2) systems analysis is much more an imaginative but practical approach to making realistically improved decisions than it is an elegant formalism.

Although systems analysis has developed a number of rules of thumb for approaching decision-making in the face of complexity, uncertainties, and limited rationality, there are still a number of unresolved questions and considerable room for improvement. For one thing, systems analysis is in need of a theory. As currently practiced

it is very much an art -- a particular style of looking at strategic planning decisions. This makes it very hard to teach except by actual experience and guidance by an experienced analyst, and it makes it very hard to set standards of excellence for analyses. It is only a slight exaggeration to say that good systems analyses are what good systems analysts do.

Many specific unresolved questions about how to do good analysis can be found in Hitch and McKean and in Quade. Some that have not received so much attention but seem particularly important are: Should analyses be structured along dimensions that are the most relevant to the interaction of ends and means or along dimensions that the decision-maker feels most competent to make judgments about? The emphasis in systems analysis on asking the right question is certainly important, especially in the long run; but for a specific decision, the result may be that the question is stated in terms the decision-maker's judgment is not oriented to cope with. It is true that one of the purposes of systems analysis is to educate the decision-maker, but that is begging this particular issue. This specific question relates to a much deeper conflict between analysis as an aid to the responsible decision-maker and as an objective and impersonal exposition of the decision to be made.

A second question also concerns how the decision-maker's judgment can best be integrated into the analysis. Specifically, are his judgments made more relevant by presenting him with refined tradeoffs

to choose among or by using his direct and a priori estimation of marginal costs and tradeoff parameters as inputs to a formal analysis? This depends on the situation of course, but it also depends on the sophistication of the decision-maker -- and it is not clear which approach is appropriate to the more sophisticated decision-maker.

A third question relates to the use of analysis in reaching a choice as opposed to setting out the explicit rationale for that choice. It is reasonably clear that these two activities are different. The former involves a creative and discerning synthesis of goals and alternatives and the finding of insights into the relationships between the two. The latter is much more a demonstration of the connections between the chosen (or recommended) alternatives and the boundary conditions of the decision. The latter, evaluative, role of systems analysis is its most often cited characteristic, and through this type of analysis the analyst and the decision-maker often get the insights for the former activity. But just what kind of analytic principles are most useful in the synthesis activity is not really clear.

A final question is how we can decide when the analysis presents a reasonably sound basis for decision and when the analysis is so sketchy that the decision-maker should rely essentially on intuitive decision. These are not clear-cut alternatives of course, since it is always a matter of degree to what extent analysis captures the relevant factors. And even when analysis can capture only a part of the problem, it is useful to the decision-maker in reducing the range of factors

that his unaided intuition must cope with. The problem comes when analysis has asked what seem to be the right questions, quantified and related to one another many of the relevant factors in terms of proximate measures of effectiveness, and yet is not able to come to grips very firmly with some stubbornly unquantifiable (but important) factors that cannot be related to the basic structure. If we assume there is some underlying "best" decision (which we pointed out in Chapter II is not altogether obvious), then how do we know when to rely on the suggestions of the partial analysis and when on the intuition of the decision-maker? This is an important problem in spite of its difficulty; real decision-makers must deal with it frequently, and there is little in the way of theory to guide them. An analogy that may help clarify this issue is the choice of betting on a master chess player or a computer that has been programmed to play chess -- before we have the chance to get statistics on their won-loss record. The computer programmer has set down all the explicit information he can extract from many masters and incorporated it into his program along with all the relevant statistics and heuristics that are known. If the reader is inclined to bet on the master, he should recall that computers are already the master at checkers and are gaining on chess; if he is inclined to bet on the computer, he should recall that past programs have not succeeded in regularly beating the masters. If he is uncertain how he should go about deciding, he probably understands the dilemma.

B. Analysis and Organizational Behavior

Strategic decision-making is affected not only by the limited capabilities of the analyst and the decision-maker in the face of extreme complexities and uncertainties, but it is affected as well by the organizational environment in which it is carried out. As pointed out above, systems analysis is reasonably well-adapted to human cognitive limitations, although this is probably more the result of pragmatic evolution of the art rather than systematic deduction from the characteristics of those limitations. Much less is known, however, about how systematic policy analysis interacts with the characteristics of organizational behavior. Cyert and March have been able to describe some aspects of how economic and organizational factors interact in the firm, but they focused neither on the strategic planning activity nor on situations where explicit attempts at optimization were being made.

One of the major considerations introduced by the organizational setting for decision-making is that some measure of defensibility is required. On the other hand, full consensus is not required. All that is necessary is that a sufficiently large number of sufficiently important people in the organization become convinced of the desirability of a particular decision. This in turn depends on three factors: (1) how do a collection of people achieve a common ground for discussing the desirability of alternative choices; (2) how do alternatives reach the point of choice; and (3) how do coalitions form in the evolution of the decision process?

What are important considerations in the strategic decision-making process are what enough of the right people accept as important. This acceptance may be based on more or less evidence and explicit rationale, but often it is based simply on the prevailing atmosphere. As systems analysis becomes more widely used, we can expect analyses to play a wider role in the determination of the factors that are accepted as significant for establishing the defensibility of a particular choice. More specifically, one might foresee as a useful byproduct of systems analysis the development of a widely shared Image in the sense of Miller, Galanter, and Pribram. Until systems analysis is more widely used, however, defensibility will have to rest largely on building a strong case for a decision in the context of those factors that are generally accepted as important or expending the effort to change the Image people hold.

Changing people's concepts of what are important factors and what constitutes a convincing argument is more difficult than most people appreciate. It is based on considerations deeper than just whether or not people can be made to understand some particular point. The Image, the Plans, and the meta-Plans that determine how a person interprets the world are the result of years of conscious and unconscious experimentation and successes and failures. They evolve not so much as the result of explicit rationale as through a settling-in process over time in which the person gets accustomed to and comfortable with them in relation to all the other Plans, meta-Plans, and aspects of his Image. Changing such a complex pattern is very difficult, and it can be very

uncomfortable unless the change happens to complement existing patterns. Further, people test Plans and Image relationships not only against their own standards, but against those of other people. We can begin to understand why an old idea can be so stable even in the face of strong "rational" evidence against it: When a person or, more significantly, a group of people get a concept "settled-in", it requires a large number of rearrangements of related stable relationships that have evolved around it if the new idea is to be accepted and not merely understood. The ideas that the sun is the center of the solar system and that heavier things fall faster were very much "obvious" because of the large number of stable relationships that evolved that were compatible with them. They were subjected to so much "irrational" opposition in the face of strong evidence not because the opponents could not understand the case being made, but because to accept such ideas without a vast amount of hedging would have thrown whole patterns of thought and behavior into question. Strategic planning decisions are not quite so cosmic in their implications, but they have a much more direct influence on the life of the people participating in the decision process.

The second question was how alternatives reach the point of choice. If the idea is accepted of effectiveness measures as devices to help us reach decisions rather than as <u>a priori</u> ends unto themselves, along with the idea that objectives get defined through the available alternatives, then the process by which alternatives are generated and filtered becomes a very important part of the decision process. Cyert and March

have included in their theory the concept that search creates commitment to alternatives, and Aharoni in his study of the foreign investment decision process confirms this. Neither, however, go into much detail about why this comes about, and that is essential if we are to devise improved decision procedures.

We have already conceded that thinking up new and better alternatives is a difficult process for which there are no particularly useful prescriptions -- except to try. It is not a process we can carry out systematically, and the growing interest in heuristics is a recognition of this. "Discovery consists precisely in not constructing useless combinations, but in constructing those that are useful, which are an infintely small minority" [41]. We can, however, say something about how alternatives, once found, get to the evaluation stage of analysis.

Basically we have argued that the search for useful and significant alternatives is intimately tied up with the evolution of goals. It is only natural that in searching for alternatives we evolve goals and hence preferences for some alternatives over others. This preference is one dimension of commitment -- it would be unreasonable to expect individuals or suborganizations to ignore preferences that they regard as responsibly based. Too often there is the tendency to regard the preferences of those at a lower level in the organizational hierarchy as capricious, when in fact they are simply doing what their sense of responsibility and their view of the facts suggests is best for the organization.

Another dimension of commitment is the uncomfortableness at lower levels of option preservation by the next higher level. Carrying along a number of alternatives creates uncertainties for the next lower level of the organization; and since these are frequently the levels that develop and screen the alternatives to be considered, there is an inevitable pressure toward commitment in order to reduce the uncertainty. This may be operational uncertainty that makes their planning for future operations very difficult, or it may be uncertainty about their future influence, power, status, or role. Still another reason for commitment is that good ideas are hard to come by. The expected gain from more extensive search is frequently quite low and is often perceived to be even lower. This is especially true in comparison to patching up existing alternatives -- an activity easier to get started on than is creating bright new ideas.

A final reason for the tendency to early commitment is that the people and suborganizations performing the search find it easier to assess the acceptability of specific alternatives than of the uncertain outcome of some future analysis. Organizations can rally around a tangible alternative more easily than the "vast hedge of preserved options" that may be more desirable at a higher level of the hierarchy. And it is important for suborganizations to be able to rally energetically, for they are the ones who must implement the final decision and who must be relied on to a large extent for new ideas.

Commitment can be viewed as an undesirable restraint on the options open to the decision-maker at the top of the hierarchy, or it can be viewed as a dedication to be capitalized on. Although the former is often accurate as a result of excessive attention to petty local objectives, it would be too cynical to argue that this is the predominant cause of commitment. It is more probable that men in the higher echelons of a large organization see themselves as responsible guardians of the organization's "true" purposes. The problem arises because they have developed Images, Plans, and meta-Plans based on the limited information available to them. As Neustadt had noted, "One need not denigrate such men to explain their conduct. For the responsibilities they felt, the "facts" they saw, simply were not the same as those of their superiors; yet they, not the superiors, had to decide what they would do" [42].

The third factor influencing standards for the acceptability of a decision is the way in which coalitions influence the evolution of the decision process. Simon's concept of the decision to participate, the either-or nature of his inducements-contributions balance, and the view of coalitions suggested by Cyert and March present an overly simplified and static view of coalitions. Rather, we should expect that the coalition structure within an organization will be complicated, rather subtle, and overlapping. Each person and each suborganization at any given time would be in a number of coalitions formed for various ad hoc purposes. Some of these can be expected to be relatively stable,

as for example the Naval aviators within the Navy, while others will be very delicate, such as the lining up of votes on a controversial bill in Congress.

We should expect conflict within the coalition to be resolved only to the extent necessary to form and maintain the coalition. Some coalitions may be quasi-permanent, while others are strictly ad hoc combinations formed for a particular decision. Some coalitions will be based on personalities, while others will be based on positions within the organization. In all these cases, we can expect that people will learn through experience what are viable coalitions or types of coalitions, and will try to repeat past successes in forming new coalitions. This type of behavior would produce the appearance of a relatively stable coalition structure within an organization over time, and would constitute relatively economic behavior given the extreme uncertainty and lack of a theory for coalitions.

Coalitions exist because they simplify the problems facing the participants in the decision process. They reduce the amount of bargaining that must be done to produce an acceptable alternative and provide environments where analysis can be done relatively free from bargaining considerations; they also provide reinforcement of the members' conceptions when formed around common views or interests. Like the Images, Plans, and meta-Plans mentioned above, coalitions are expensive to disrupt. We can expect them to be violated and rearranged only if less disruptive alternatives cannot be found. Cyert and March

emphasize the use of side payments in the establishment of coalitions. Simon postulates that once a reasonably acceptable alternative has been found, search will be shifted to the identification of side payments that will establish a coalition sufficient to assure the choice of the alternative. Many of these side payments are in the form of policy concessions -- concessions on the performance of the alternative. However, we can expect that the participants in the decision process often will perceive it to be less costly in terms of organizational goals to compromise, make the side payments, and establish the necessary coalition, than to try to convince all the key participants of the objective efficiency or effectiveness of a particular alternative.

Coalitions are necessary because people differ in their views of the structure underlying the decision, in the types of arguments they regard as convincing, in their values, in the information available to them, and in the particular set of ends-means premises they bring to the decision. A particularly important difference among people that makes coalitions -- rather than objective argument -- the more efficient way of reaching decisions is how different people react to experience and what they "learn" as the result of experience. The significant differences are not the specific data that people learn but in the types of things they learn. For example, some people will learn specific facts. From the same experience, others will remember processes -- what happened and what were the interactions. Still others will learn programs -- techniques and operational arrangements that were most useful. And others will learn heuristics -- criteria for judging what

kinds of techniques will be most useful in a new situation. The latter type of individual is particularly valuable in the strategic planning process, but this capability will not necessarily permit him to convince the other participants in the process of the desirability of his views.

Because of all these differences in patterns of thinking and standards for what constitutes a convincing argument, we should expect that coalitions will tend to seek alternatives, rather than goals or structure, as a common focus. Objectives are hard, if not impossible, to get agreement on. Even so, if people could agree on structure we might expect a rather higher level of discussion than is achieved with bargaining via coalitions and side payments. But they often cannot -- or do not -- and this added difficulty of agreeing on structure leads to a bargaining over what structural relationships should be employed as well as bargaining over objectives and specific alternatives.

C. Organizational Reactions to Systems Analysis

Based on the ideas developed so far, we can predict a number of ways people in organizations will react to the introduction of systems analysis into the strategic planning process. Explicit analysis of policy alternatives represents a communication and decision-justifying process that is alien to many participants in the decision process. Simple unfamiliarity with the language and the tools of systems analysis

is no doubt one of the major obstacles to its wider acceptance. Busy men are not readily disposed to take the time and effort to master some relatively abstract concepts, and even if they were it would be very difficult for most of them to integrate these new concepts into the patterns of thought they have evolved over a lifetime. This lack of understanding also contributes to poor analyses that reinforce the initial bias against systems analysis.

As a new way of looking at decisions, systems analysis represents a threat to the power and influence of those not well-versed in its techniques and language. If they concede that it is a useful way of addressing the strategic planning function of the organization, they are in fact conceding to those who are better versed in it an advantage in the bargaining process.

Since systems analysis tries to be explicit and objective about assumptions and measures even when arguing for a particular alternative (rather than setting out with a preconceived choice), there is the decided possibility that the analysis will produce results contrary to some of the predispositions of the participants in the decision process. Some people view this as a useful result because it provides a basis for improving and sharpening their judgment. But others will interpret it as a challenge to their judgment and hence to their competence and status.

By cutting across established organizational and coalition boundaries, analysis increases the uncertainty about how a particular individual, suborganization, or coalition will fare in the final decision. Program budgeting, even without explicit analysis will have similar effects. As we mentioned above, this is also one of the reasons coalitions form around alternatives rather than objectives.

We referred earlier to the distinction between reaching a decision and setting out the rationale for that decision. The cognitive processes in these two activities are different, and systems analysis makes this difference more obvious by the use of an explicit decision-justifying language that is basically incompatible with the way most people reach their decisions. As long as people need not be too explicit about their justifications, this problem can be glossed over. But systems analysis makes it difficult to ignore, and people may be made quite uncomfortable by having to justify their positions on ground rules different than those they used to reach the decision in the first place.

By its emphasis on explicitness, systems analysis forces people to face issues they were able to gloss over previously without even being aware that they were doing so. In writing about the development of radar, C. P. Snow observed [43]: "Even at the highest level of decision, men do not really relish the complexity of brute reality, and they will hare after a simple concept whenever one shows its head." Simplification facilitates dissonance reduction by avoiding the issue

of opportunity costs. It reduces interpersonal and inter-organizational conflict by being explicit only about those things that must be made explicit -- and the bare minimum on this scale is the chosen alternative. But as we have pointed out above, simplification can also be a source of conflict because each person simplifies differently. Unfortunately, the kind of simplifications used in systems analysis are different from the kinds that facilitate the bargaining process.

Another reason for adverse reaction to systems analysis is that people do have a sincere desire for reaching the best decision possible -- and by calling explicit attention to the uncertainties involved, systems analysis calls explicit attention to the limited basis for decision that many people find uncomfortable. It is unsatisfying to many people to speak of tradeoffs among objectives since that implies compromise with what we "really need". This is one of the reasons bargaining tends to be in the language of problem-solving rather than of values. By addressing many alternatives and stressing Pareto-optimal alternatives, systems analysis makes it more difficult to insist on the "best" alternative or on "needs" that ignore the tradeoff between cost and performance.

Advocates of systems analysis stress its use in clarifying ends-means relationships and sharpening the judgment of the decision-maker. But we have seen that clarity in ends-means relationships and on objectives may make for increased disagreement within the organization. By stressing the uncertainties and the links to objectives, systems

analysis may make the decision-maker feel less confident that he has made the right decision than he would have without it. Little clarity enables the participants in the decision process to avoid the sense of personal responsibility and organizational vulnerability by couching their thinking and their defense of their decisions in technical, problem-solving terms rather than on fundamental tradeoffs among objectives. They are then vulnerable to criticism only by those few people in a position to question technical competence and value choices.

A final reaction to systems analysis concerns the locus of judgment. In systems analysis, judgments are regarded as part of the decision problem, and the emphasis is on clarifying specific judgments that must be made and using analysis to relate them to one another. The more common approach is to present the "facts" of the problem and let the decision-maker make a lumped judgment at the time of choice. The net result of this difference is that with systems analysis it is much more difficult for the decision-maker to foresee the tangible implications of his judgments, while under the lumped-judgment approach he has full control over the outcome of the "analysis". This control may be used to assure that a favored alternative comes out ahead, but it probably is more commonly used as insurance against unreasonable results from the analysis. People simply are able to assess the implications and acceptability of concrete alternatives better than they can assess the outcome of judgments separated from analysis. Since a major function the decision-maker performs is to integrate diverse considerations rather than to delve into the details of each, this

shift of the locus of judgment may make it difficult for him to justify a valid but vaguely formulated overview in the face of an analysis that factors the decision structure into a number of sub-areas.

D. Uses and Limitations of Systems Analysis

Analysis can be used in two ways: in reaching a better conceptual understanding of the problem and deciding what should be done, and in the bargaining with other participants in the organizational process of deciding what will be done.

In decision-reaching, the purpose of analysis is to provide the decision-maker with information that will improve the basis for decision. One of the major uses of systems analysis in this context is the exploration of the relationships among objectives and alternatives. The alternatives that are available determine to a large extent what are desirable combinations of objectives. In decision-reaching, analysis can be used to suggest new or improved statements of objectives; to help clarify the decision-maker's understanding of the structural relationships of the problem, including particularly the linkages between the objectives and the control variables; to suggest improved alternatives; and to provide information evaluating the alternatives against the objectives as a basis for choice by the decision-maker. Through a continuing cycle of analysis and judgment, the decision-maker can use analysis as a sounding board for

improving his understanding of the issues, sharpening his judgment, clarifying and improving his objectives, and obtaining high quality alternatives to choose among.

The most severe limitations of analysis in decision-reaching have been cited above. An additional important limitation is the interaction between the decision-maker and the systems analyst. Counter-intuitive and seemingly paradoxical results are not likely to be accepted unless made intuitive to the decision-maker. This means that a close relationship between the decision-maker and the analyst is required. It would be desirable for each to understand the problems and the thinking of the other, but there will be an inevitable mismatch. Just how to integrate most effectively analysis and judgment is very much tied up with this interpersonal interaction, and we can expect that failures of communication here will severely limit the usefulness of the analysis.

We have argued that systems analysis cannot change completely the way people think and interact with one another in the strategic decision-making process. It is just one more consideration for the principal participants in that process. Therefore, we should expect the bargaining process to remain the context within which decisions are made, although we should also expect the bargaining process to be changed by the introduction of systems analysis. We have suggested above many of the impacts that could be expected. But there are specific uses we would expect to see made of systems analysis by the

participants in the process. If we still accept the view that most of the participants are sincere, responsible men, we would expect that a major attempted use of systems analysis in the bargaining process would be the education of the other participants. Regardless of the extent to which a participant relied on analysis in reaching his opinion of what is the proper decision, we can expect him to use any analytic resources he has to assure the acceptance of that decision. Some of the ways it can be used in this regard are: confuting, embarrassing, overwhelming, stalling, and sidetracking through tangential or very complicated analyses.

The limitations of analysis in the bargaining process appear to be due to the principal participants' differing perceptions of the environment and of the problem in relation to it. Their differences in thought patterns, concepts of convincing argument, organizational responsibilities, and loyalties also limit the extent to which analysis can be constructive in the bargaining process. Analysis tends to make these differences more explicit. While it improves the informational basis for decision, analysis may make the tensions and strife among the principals sharper along with their judgments. Finally, analysis usually omits issues of power and leadership because they cannot be fitted into the analytic framework. It would be desirable to include them since they are key components in many decisions. With our limited understanding of such issues, however, it is not clear whether this ommission is a limitation or a virtue.

E. Systems Analysis vs. the Bureaucracy

So far, we have explored how systems analysis and organizational behavior characteristics can be expected to interact. We have developed some insights into why there may be a significant reaction against systems analysis as well as why it can be useful to those who appreciate its capabilities. What are the implications of these insights; how can we use them to encourage the use of analysis in the strategic planning decision process and to get around the inhibiting effects of adverse reactions?

The least effective way to promote heavier reliance on systems analysis probably is to appeal to the reason of the people in the organization and to the reasonableness of systems analysis ideas. The closest substitute would be to show that systems analysis produces demonstrably better decisions. Since we have argued the intertwining of goals and alternatives, however, this approach is not likely to convince many people unless they are already convinced. Institutional forms such a program budget categories and models built into the information gathering and display system will have some effect. Since people structure their thinking to fit the information available, making available information that suggests connections between resource allocation and objectives is bound to influence the approaches taken to analysis.

Some sort of organizational incentives to encourage good analysis would be very desirable, but if these are too strong they may produce more resentment than the desired results. Because of the fundamental

difference in approach between systems analysis and more conventional ways of reaching decisions, early-in-career training is probably the most effective way of encouraging the use of systems analysis. The principal decision-makers are much too busy to do much analysis themselves, and if their analysts are structuring decisions along the desired approach, there is bound to be an improvement. (And these young analysts will eventually become direct participants in the decision process themselves.) Promotion incentives can be an effective device, but they can also create considerable antagonism among those bypassed.

The organizational strife produced as a result of imposing a systems analysis framework on the strategic planning decision process is not so clearly all bad. It is particularly true at the strategic planning level of an organization that enthusiasm and vitality thrive on support and opposition. A reasonable amount of disagreement can be very productive in assuring that we do not get trapped in specious analyses and in providing the impetus for the development of improved alternatives. The only real question is how to decide how much of this tension is desirable and how best to harness it.

Finally, systems analysis must begin to take into account organizational considerations. In the face of glaring misallocation of resources, this is not so important. But as we refine our allocations and our analyses, these considerations are going to become relatively more important. Systems analysis is going to have to

consider that the enthusiasm of commitment is a part of the opportunity of an alternative and that there is a place for the inspirational, heroic decision as well as the decision that produces a vast hedge and preservation of options.

F. An Overview

It is not possible at this time to organize all the observations of the previous pages in any very satisfying compact way. There are just too many ways to organize them and to relate them to one another. We can, however, summarize what seem to be the most significant aspects of the point of view we have taken.

1. Systems analysis is basically the current state of the art for attempting to be as effective and efficient as possible in the resource allocations of the strategic planning function. While striving toward the idea of optimization, it is very much adapted to cognitive limitations in the face of complexity and uncertainty.

2. Systems analysis is but one of many resources available to the decision-makers and but one of the ways of looking at a decision that are competing for his attention. We can expect that it will be but one (often major but often minor) contribution to his final choice.

3. Objective rationality in organizational decision-making is not possible because of differences among the participants in the decision process in their values, their conception of the structure underlying the decisions, and their criteria for what constitutes a convincing

rationale. Analysis will be carried out in the context of a bargaining process and will be fitted into the organizational decision process through the bargaining among the principals. As the quality and relevance of analyses improve, we can expect a shift toward bargaining within the context of the analysis instead of the other way around.

4. Because systems analysis is part of a larger decision process, not coterminous with decision-making, we can expect it to be used for a variety of purposes:

a. Exploration of ends - means interactions

b. Clarification and improvement of objectives

c. Comparison of alternatives

d. Generation of new alternatives

e. Providing a framework for discussion

f. Providing a bargaining advantage

g. Embarrassing, stalling, confusing, educating...

5. We can expect bargaining over influence and suborganizational objectives to continue to be based on problem-solving terminology. Introduction of systems analysis as the framework of discussion can be expected to make this type of bargaining more difficult. This should in turn make this less influential on the decisions of the organization, but may either increase or decrease the effort devoted to it.

6. By shifting the locus of judgment from the point of choice to various specific points in the analysis, the introduction of systems analysis into the decision process will reduce the sense of control decision-makers feel they have over their decisions. By attempting to avoid premature commitment to specific alternatives, it will make coalition formation more uncertain and may act to dampen enthusiasm for the creation and development of new alternatives.

7. By its emphasis on explicitness, systems analysis can be expected to increase the subjective sense of difficulty of some decisions by sharpening value tradeoffs, and by increasing vulnerability to criticism.

8. Systems analysts and organizational theorists need to examine these ideas more closely in order to make systems analysis more useful in the decision process and to develop organizational arrangements and incentives that will avoid some of the difficulties.

CHAPTER V

CVA-67: Conventional vs. Nuclear Propulsion

The CVA-67 was the attack carrier in the FY 63 budget. Although the Navy requested that it be a nuclear-powered carrier, Secretary McNamara made the decision to ask Congress for a conventionally-powered ship. This was authorized and funds were appropriated for the FY 63 budget. Because of the advance planning required for budgeting, these decisions were made in 1961 and 1962. Subsequently, the question was raised in 1963 of whether the CVA-67 should be changed to nuclear power. This chapter describes the decision process during 1963 leading up to a decision by Secretary McNamara not to ask Congress for authority to make that change.

The reconsideration was first raised on 7 January 1963 in a letter to Mr. McNamara from Dr. Glenn Seaborg, Chairman of the Atomic Energy Commission, which conducts a research program on reactor technology for naval warships. He noted the development of a new reactor that would permit a four-reactor carrier power plant instead of the eight reactor system used in the CVAN-65 Enterprise, the first and only nuclear carrier. He stated that the Commission felt a review of the future of nuclear power for surface ships was needed for planning

purposes and raised the question of whether it was too late to reconsider the decision to make the CVA-67 conventional. In support of this action, he noted the experience that had been recently gained with the Enterprise and two nuclear-powered escort ships, the reduced fuel costs of the new reactor, and the advantages of nuclear over conventional propulsion.

On 23 January, the Secretary of the Navy, Fred Korth, wrote Mr. McNamara, citing the Seaborg letter. He concluded that the four-reactor nuclear plant should be substituted for the conventional power plant in the CVA-67. This conclusion was based on the following factors:

1. A listing of a number of operational advantages of nuclear power over conventional (e.g., sustained high speed for reduced vulnerability to submarine attack, longer aircraft life due to elimination of stack gases, etc.), with no indication of the magnitude of these advantages.

2. Statement that the best way to reduce the cost of nuclear power is to expand the nuclear shipbuilding effort.

3. The new reactor as an improvement over reactors available when the original decision was made.

Some quotations [44] from this memorandum illustrate the way in which the elements of the problem were organized to support the recommendation:

> ...cost is the major factor in determining whether nuclear-powered surface ships will be built by the Navy as compared with conventional propulsion.
>
> We are aware that it is difficult to assign a dollar value to the military advantages of a nuclear carrier. The tremendous number of significant operational advantages, offensive and defensive, for both task force and individual ship operation that result from the virtually unlimited cruising range, long endurance, sustained speed, structural improvements, superior electronic performance, tactical flexibility, and freedom of movement of the nuclear carrier are certainly worth a considerable premium.
>
> It is apparent that a cost premium in the short term is inescapable in order to keep alive technical and manufacturing progress, and AEC interest, toward the goal of cheaper nuclear ships in the long term.
>
> As a result of my review of all pertinent considerations, as highlighted by Dr. Seaborg's letter, I conclude...

The Navy and the AEC clearly had a common interest in reversing the decision; both pointed out the impact of such a reversal on the impetus toward an all nuclear Navy. The two most significant points about the Korth memorandum appear to be (1) that it urged the decision on the CVA-67 as a step toward commitment to an all nuclear Navy and (2) that, in spite of having defined clearly that the major criterion for decision was whether the added performance was worth the added cost, it offered no rationale for the conclusions: "As a result of all pertinent considerations, ... I conclude ..."

Mr. McNamara agreed to review the decision and had members of his staff confer with people in the Navy on the matter. On 22 February, he wrote Mr. Korth:

> I do not feel that the subject of nuclear propulsion for surface warships has yet been explored sufficiently to permit a rational decision.

Regarding Mr. Korth's argument of the effect of a decision to go nuclear on the impetus toward a nuclear Navy, he noted:

> These are forceful arguments, but I am sure you realize that they depend upon the assumption that the future Navy will, indeed, make full use of nuclear power. It is precisely this question which lies at the heart of the matter; far more so than the question of whether CVA-67 itself should or should not be nuclear powered.

Finally, he spelled out in detail the kinds of analysis he regarded as necessary to permit a decision on whether the increased performance resulting from nuclear propulsion was worth the cost:

> Accordingly, I should like you to undertake a comprehensive, quantitative study of this matter. This study should consider the design of the future carrier striking force in the broadest possible context. You should consider the implications of nuclear power on the composition of the task force. How many escort vessels of what type should be included? *** How is replenishment of aviation fuel and ordnance to be accomplished? *** How should the Navy be deployed around the world? *** Realizing that we will have a large number of conventionally powered surface vessels in the inventory for some time to come, how should we approach the "ultimate" design? *** What are the implications on force size? Would nuclear propulsion allow us to reduce the total number of carriers and/or carrier task forces?

> As a general guide, I am interested in achieving the most efficient possible naval forces, defining efficiency as achieving the most beneficial military results for a given expenditure. If nuclear propulsion permits an increase in this efficiency, then advantage should be taken of it. However, I do not feel that a proper evaluation of such possibilities can be made in the absence of a thorough and comprehensive study which goes beyond the narrow consideration of CVA-67 alone.

As a result of this memorandum, a study was done within the Navy to determine the extent to which nuclear propulsion should be incorporated in future surface warship construction and was completed on 9 April. The study examined several alternate task group compositions and several alternative employment concepts for these task groups. Unfortunately, the selections emphasized severe demands for endurance (in order to highlight any differences between nuclear and non-nuclear forces), so that the study was biased in favor of nuclear carriers. It is not clear whether the bias was intentional or simply reflected a misguided methodology. Much attention was given to the detailed examination of alternative supply and operational possibilities. Once again, however, the advantages of nuclear power were simply listed, and the extension from this list and the detailed operational possibilities to the conclusion was not justified by any explicit rationale. After describing the myriad of factors with little indication of their relationships or significance, the conclusion is simply that the operational gains to be achieved from nuclear power in attack carriers are ... "substantial and significant". On this basis, the study recommended that all CVAs should be nuclear beginning with the CVA-67.

On 4 April, Mr. Korth replied to Mr. McNamara's request for a comprehensive and quantitative analysis of the decision, basing his reply on this study. Given the study on which it is based, it is not surprising that this memorandum is little more than an expansion of his previous argument. A longer and more detailed list of the advantages of nuclear power was presented as an enclosure as was a list of answers to the specific questions raised by Mr. McNamara. In some cases the questions are talked around rather than answered, and where they <u>are</u> answered directly, no rationale is presented. For example:

> Studies have been conducted on...

(no indication of methodology, assumptions, etc.)

> These studies indicate that, for the foreseeable future, the cost increase of ... does not appear justified on a cost effectiveness basis.

(no indication of why, or what "cost effectiveness" means).

The text of this Korth memorandum is based on three points:

> (a) The military gains to be achieved from nuclear propulsion are substantial and significant. It is difficult to place a precise dollar value on many of these gains, as many of them can be achieved in no other way.
>
> (b) The operational experience with Enterprise, Long Beach, and Bainbridge has proved the outstanding capabilities and reliability of their nuclear propulsion plants.

> (c) For nuclear-powered warships, the military effectiveness in relation to cost exceeds that for their oil-fueled counterparts despite the fact that individual nuclear surface ships will cost more than their conventional counterparts with the same armament.
>
> Based on the considerations outlined above, the Chief of Naval Operations and I support the policy of nuclear propulsion in all new major combatant surface ships (larger than 8,000 tons displacement) and of research and development programs directed toward ultimate wider introduction of nuclear power in surface warships.

At this point, the most significant factor seems to be that the Navy either refuses to be explicit about the rationale for concluding that the operational advantages of nuclear power are worth their cost, or accepts ringingly positive statements and lengthy lists of advantages to be a sufficient rationale. Alternatively, Mr. Korth finds it easier to give the admirals their nuclear ships and to devote his efforts elsewhere than to subject their impressionistic preferences to a rigorous inquiry that could prove embarrassing. In any event, he clearly expects McNamara to accept his judgment about the matter and not to insist on a detailed rationale.

Mr. Korth was quite mistaken on this expectation. On 20 April, McNamara replied to Korth's memo of 4 April:

> Your memorandum does not provide me with the information I need in order to reach a decision on this important matter.

> Specifically, my question concerning the implications of nuclear power for force size has not been answered. You state that nuclear propulsion permits a significant increase in beneficial military results for a given expenditure and you note that the benefits may be taken in the form of either reductions in carrier task forces or increased effectiveness, but you have failed to identify the magnitude of the increase in effectiveness or the possible reduction in force. Thus, I am asked to consider a course of action which would, among other things, add at least $600 million to the 5-year shipbuilding program without knowledge of the ultimate effect of these outlays.
>
> Similarly, my question on the implications of nuclear power for the composition of task forces has not been answered.
>
> Without unambiguous answers to these two questions, the approximate impact of the nuclear-power program you recommend on other naval programs and the defense budget cannot be determined.
>
> In addition to these two major points, I feel that some additional clarification of your analysis should be possible. While I realize that there are many issues involved here which are not subject to rigorous quantitative analysis, a systematic exposition of those issues which are quantifiable is necessary if I am to appreciate fully your position.

In addition, he asked to see the "more recent analyses" Korth had referred to and spelled out in even more length and detail than in his previous memorandum the type of analysis he wanted done before making the decision.

It would be difficult to imagine a more specific statement of what the Secretary considered necessary in the way of analysis in order to make an intelligent decision than was spelled out in this memorandum.

His two emphases were: (1) convincing the Navy of the critical importance of explicit demonstration that the added cost of nuclear power is indeed compensated for in added effectiveness and (2) laying out in detail the steps of an analysis that would provide some approximate answers to that question. On the former point, he stated very clearly just what his view of the basic structure of the problem was:

> Of course nuclear-powered ships are better than conventional ships, cost not considered. But cost has to be considered because it is a measure of what is being given up elsewhere -- elsewhere in the Navy, the Department of Defense, the Federal Government, and the economy as a whole. The absence of arbitrary budget ceilings does not mean that resources are unlimited. I need to know whether nuclear power for surface ships is a sensible expenditure as part of any budget, or whether your proposal merely makes sense if the implied reductions in other capabilities are neglected.

In specifying what he felt was necessary in the way of analysis, McNamara noted that forces of equal cost or equal effectiveness were necessary for comparison purposes and that a sensitivity analysis on the number of escorts per task group was necessary to see if task force composition would change the choice between the convertional and nuclear carriers. His specification of the analysis to be done was in three categories: long-run comparisons, short-run problems, and analysis of effectiveness. For long-run comparisons, he asked that the following table be completed under four conditions:

Conventional Force		Nuclear Force, effectiveness equal to conventional		Nuclear Force, cost equal to conventional	
Composition	Cost	Composition	Cost	Composition	Cost
.	.	.	.	.	.
.	.	.	.	.	.
.	.	.	.	.	.

The four conditions were:

(1) Four escorts per carrier

(2) Eight escorts per carrier

(3) Four escorts, not to exceed 5,000 tons, per carrier

(4) Eight escorts, not to exceed 5,000 tons, per carrier

The conventional carrier force used as a base was to include 15 carrier task forces. The analysis of short-run problems was to include a projection over time of the cost of maintaining current capabilities with conventional ships; the cost with a transition to nuclear ships; and the effectiveness over time during a transition to nuclear ships at the same rate of cost required to maintain current capabilities with conventional ships. His suggested analysis of effectiveness emphasized the need to know the approximate magnitude of the importance of the advantages of nuclear power that Korth had repeatedly listed. Quotation of one segment of these suggestions will illustrate the approach he detailed:

> The use of scenarios should also enable you to demonstrate the points made in enclosure 1 of your memorandum. For example, you note the higher speeds of nuclear ships and their freedom to engage the enemy immediately upon reaching the combat area as an advantage. The scenarios should allow you to calculate, assuming given initial dispositions and given launch points, just how much sooner the first strikes could be delivered. Of course, you will also have to calculate differences in the buildup of sorties conducted as a function of time, accounting for any differences in total number of embarked attack aircraft.

Finally, to indicate that he wanted to see an explicit statement of rationale, rather than just the results, McNamara concluded:

> Of course, the relative effectiveness of the two forces will depend on the assumptions made. I want to know how the assumptions affect the conclusion and I want to know what assumptions are required to show that nuclear-powered forces are superior to conventional forces of equal cost.

After this second refusal to accept the Navy's recommendation that CVA-67 should be nuclear and this very explicit rejection of the first study done by the Navy, the Navy turned to its captive non-profit analysis firm, the Center for Naval Analyses, for assistance. A study was conducted over the summer of 1963, designated NAVWAG 28. Analysts on Mr. McNamara's staff were aware of some of the methodology and results of this study on an informal basis, but the study itself was not furnished to his office until November. One of its conclusions was:

> An overall evaluation of the superiority involves value judgments of the type which the Center for Naval Analyses has refrained from making in this study. Thus the question, in its broadest form, is not answered here.
>
> With the evaluation restricted to the measures of effectiveness employed in the analyses and with the assumptions limited to those which could be properly and reasonably utilized by the Center for Naval Analyses, no set of assumptions could be found to show that nuclear-powered forces are superior to conventional forces of equal cost.

The Navy was clearly unhappy with this analysis. The unhappiness could have been based on the methodology involved, but it is understood that much of the methodology was used without change in a subsequent in-house Navy study. The unhappiness seems rather to be the result of their view of the role of analysis in decision-making. The Navy's rejection of the study illustrates this Navy view and shows how systems analysis can interact with the "real" decision process:

> Between the Navy members and your analytic staff, there have been many minor and major disagreements over tactics, assumptions, format, and content, to the net effect that the study was not developed in consonance with the guidance offered by the Navy members of the steering committee and of the study group itself. It, therefore, does not reflect a consensus or even a majority opinion. In view of this, and the statement quoted above, that the study omitted operating judgment factors, it is considered that the study should more properly have refrained from reaching conclusions. *** Since it clearly does not represent Navy views opinions, and findings on nuclear propulsion reached by other analyses supported by widely shared operating experience, the study regrettably needs fundamental reworking based on a better understanding of naval operations in the years ahead.

Following this rejection of NAVWAG 28 by the Navy, an analysis was done by the office of the Deputy Chief of Naval Operations (Fleet Operations and Readiness). Although the methodology was similar to that of NAVWAG 28, the conclusions are far different. The significant part of this study was the cost effectiveness comparison of nuclear and non-nuclear forces. It is worth giving some attention to the methodology of this study, since it illustrates the difficulty the Navy had in grasping how quantification could be used in a decision of this sort.

Ten effectiveness factors were identified:

1. Response time (response differential)

2. Sorties (average number for first 10 days)

3. Staying power (consecutive days)

4. Embarked aircraft (number)

5. Vulnerability (unspecified)

6. Task force flexibility (freedom to dispatch independent units)

7. Readiness and reliability (safety and and construction factors)

8. Special force capability (quick strike and militant presence)

9. General war capability (surviving force after nuclear exchange)

10. Other factors (advancement of technology, modernization potential, etc.)

Numerical values were assigned to each for the conventional and nuclear powered carriers; the conventional carrier was taken to have an effectiveness of 1.0 on each measure and the nuclear carrier's measure was scaled accordingly. Numerical weights, adding up to 100% were assigned to each factor and a weighted sum of the ten measures was taken as the effectiveness of the carriers: "Important measures of task group performance were weighted and values were assigned on both an analytical and a judgment basis." The heavy weights were given to those factors where the differences between conventional and nuclear power were greatest -- presumably (as in the original study of 9 April) to highlight the differences between the two ships. The net result was a bias in favor of the nuclear ship, of course, but that is not the most significant thing about this approach.

The significant thing is that the Navy was willing to give numerical values to things like "advancement of technology" -- to say seriously that the nuclear ship was 1.25 times better on "other factors"

than the conventional ship -- and then to say that "other factors" constitute 8% of the effectiveness of a task force. (Both these numbers are illustrative because of the classified nature of the analysis.) The magic number turned out to be that a nuclear task force was 1.21 times more effective than a conventional task force. (This and the following cost figure are the actual numbers, taken from unclassified sources.) From here, the quality of the reasoning goes downhill. Since a nuclear task force including the air wing costs (according to the study) 1.03 times more than a similar conventional task force (augmented with additional oilers to make it comparable to the nuclear case), we are getting 21% more effectiveness for only 3% more cost. Now, since 1.2 x 5 = 6 and since 1.03 x 5 = 5.15, only five nuclear carriers would give the same effectiveness as six conventional carriers, and the cost savings would amount to several hundreds of millions of dollars over a 25 year period. So runs the "cost effectiveness" study.

On 26 September, Mr. Korth again wrote Mr. McNamara recommending that the CVA-67 should be nuclear. He enclosed a listing of the above ten factors and a table showing the figures of 1.21 vs. 1.0 for effectiveness and 1.03 vs. 1.0 for costs, as well as similar numbers for older carriers and for a hypothetical large conventional carrier. The text of this memorandum bases the recommendation for nuclear power on three factors:

1. Still another listing of the qualitative advantages of nuclear power.

2. The observation that five nuclear task forces will give the same effectiveness as five conventional at less cost based on the cost effectiveness studies.

3. The next opportunity to build a nuclear carrier is several years ahead.

On 9 October, Mr. McNamara wrote Mr. Korth that he had decided that the CVA-67 should be built as a conventional carrier as originally authorized and funded. In contrast to the detailed and comprehensive discussion of the analysis central to the decision in his previous statements, there is only one reference to analysis in this memorandum:

> My original intent in requesting a comprehensive study of nuclear propulsion was to expand the particular issue of the fiscal year 1963 carrier to a general policy issue. *** However, on the basis of the analysis available to date, I am not convinced that a net advantage is in prospect.

He clearly had given up on getting an explicit, worthwhile rationale for the decision out of the Navy. His own staff resources for analysis were quite limited at that time, so he had to make the decision on what in his view was very limited information.

His basic approach to the decision was to hedge:

> As a minimum, I am confident that construction of the fiscal year 1963 carrier with conventional rather than nuclear power would not result in any serious loss of effectivenss.

Both McNamara and the Navy agreed that it was important to begin construction as soon as possible on a new carrier, whether it was to be nuclear or conventional. McNamara added in this memo to Korth:

> Considering the state of the legislative calendar and the previously expressed attitudes on the subject of certain key congressional leaders, it is doubtful, to say the least, that congressional approval of a shift to nuclear propulsion for the fiscal year 1963 carrier would be either swift or sure.

He deferred decision on the general policy on nuclear propulsion and suggested that the subject should be raised again when new studies were completed.

The following day, Korth asked McNamara to review his decision. His basic argument was the judgment that nuclear power should be the basis for the Navy of the future:

> The chief of Naval Operations and I believe that nuclear propulsion does contribute to achieving the most efficient possible naval forces and that it offers outstanding advantages.

On 25 October, Mr. McNamara wrote Korth that he had reviewed the case and had discussions with several naval officers and still concluded that the CVA-67 should be conventionally powered. The thrust of this memorandum is that the available analyses are not sufficient basis for

a choice about the future propulsion policy of the Navy and that in the absence of a decision on that issue the most expedient action on the CVA-67 is to proceed on a conventional basis. His own attempts at analysis and his review of Navy analyses had convinced him of the lack of understanding by all parties of how the many factors involved related to the central question -- whether the added expense of nuclear power was compensated for by the added performance. His memorandum of 25 October, discussing how a number of the performance factors relate to effectivness in a way that shows some real thought about the problem, is a marked contrast to the Navy's ludicrous "cost effectiveness" model and unstructured "judgment" that the diverse qualitative advantages added up to effectiveness worth the cost.

A quotation of one of these points will illustrate the style of the memorandum:

> The results of preliminary studies made available to me indicate that, in your judgment, five nuclear-powered task forces are as effective as six conventionally powered task forces. While this may be true under certain specified conditions, it has not been shown that the conclusion is generally valid.
>
> Since the conventional force has 20 percent more aircraft (striking power), the argument applies only in that limited period of time during which conventionally powered forces have not arrived at the point of attack. Your studies show that after 5 days of steaming toward an objective area, the conventionally powered carrier is only about 4 hours behind the nuclear carrier as a result of having to slow down for replenishment of fuel. ***

> The history of surprise attack suggests, as noted in Navy studies, that a response should be made within 2 to 5 days if it is to be effective. For this critical period, the conventionally powered carrier appears to be quite comparable to its nuclear counterpart. In fact, since the nuclear force you envisage is smaller, carriers will be spread more thinly and will have, on the average, a greater distance to steam. This, it is entirely possible that the nuclear-powered force might have a longer reaction time, rather than a shorter one.

McNamara concludes by once again pointing out the importance of early start on construction of CVA-67, the importance of the larger decision about the future propulsion modes for the Navy, and that this decision on CVA-67 did not constitute a policy question.

CVA-67 was constructed as a conventional carrier and will be commissioned in 1968.

CHAPTER VI

Fast Deployment Logistics Ships

One of the characteristics of the buildup in general purpose forces initiated by Secretary McNamara has been an expansion in rapid deployment capabilities -- the ability to move large numbers of troops and equipment into a troubled area in the early stages of conflict. In 1961, United States capabilities for deployment of general purpose forces included air transport planes, Military Sea Transportation Service (MSTS) troopships and cargo ships, and prepositioned supplies in Europe and Southeast Asia.

When systems analysts began to look at the rapid deployment issue, there was little understanding of how to decide on the lift capability required or how it could be provided most efficiently. Over a period of several years both technology and analysis were improved, and in 1965 Mr. McNamara proposed a major large construction program for large, high speed, humidity controlled ships that could be preloaded with Army divisional equipment and moved to trouble spots as the need arose [45]. This chapter describes the evolution of that decision.

Program memoranda are prepared each year in the Defense budget process that set forth the rationale for the budget decisions. There are a large number of these memoranda and one is devoted to airlift-sealift forces. It provides a projection of airlift-sealift forces by year beginning with the year being budgeted, a discussion of the key issues, and the rationale for the force levels selected. An initial memorandum is prepared in the Office of the Secretary of Defense (OSD) and circulated to the Joint Chiefs of Staff and the armed services for comment. Based on the reclamas they file, a revised memorandum is prepared [46].

In Mr. McNamara's first year as Secretary, 1961, the airlift-sealift memorandum was little more than an essay on the decisions. In that year the MSTS troopships were to be discontinued in favor of airlift for the troops. The construction of one large, high speed Roll-on/Roll-off (Ro/Ro) ship was decided upon; this ship was designed for rapid loading and offloading of Army wheeled and tracked vehicles. The possibilities of prepositioning supplies in ships, the floating base concept, were noted in 1961, but no decisions on the concept were made [47].

By 1962 (the FY 64 budget process), the floating base idea had been made more concrete and was known as the "forward floating base" concept. A decision was made to convert old Victory ships to controlled humidity storage "Forward Floating Depots" (FFD) and to preposition these around the world. The Ro/Ro ship had not been approved by

Congress, but was still being carried as an open alternative [48]. However, the role it should play in airlift-sealift was not clear. For one thing, it was seen as a competitor to the FFDs. It was foreseen as an addition to the MSTS fleet, but the use in peacetime associated with such assignment conflicted with the rapid deployment function it was designed to serve. Lastly, prepositioning seemed preferable to high speed sealift to Europe, and the deep draft of the ship combined with beach and port limitations in underdeveloped areas made the usefulness of the Ro/Ro there uncertain. OSD suggested a redesign of the ship and/or the concept before proceeding with development of a floating depot ship optimized for that specific role. The major conclusion of the 1962 analyses was that the United States was seriously lift-limited in its conventional warfare capability and that large increases had to be made. OSD also concluded that the "Brute Airlift Approach" to rapid deployment that some had suggested was too expensive to be feasible and that a "Systems Approach" (whatever that was) would be required [49].

By 1963, no clear picture of how to approach the analysis of lift forces had evolved. The C-141 jet transport was being procured in large numbers, and a breakeven analysis suggested that a hypothetical large transport designed for lower density cargo, the CX, would be worth developing in the airlift area in spite of the sunk costs in the C-141 [50]. (The CX eventually became the C5A.) Programmed airlift capability was much greater than the 1961 level and a clearer assessment of needs and goals was seen to be necessary. The tradeoff between

rapid deployment capability and additional forces for later commitment was seen as a significant determinant of overall lift capability, and the analytic approach desired was to identify the optimum mix of airlift and sealift and the optimum mix of vehicles within a mode. In early 1964, many of the important questions had been raised that were necessary to a fully developed rationale for airlift/sealift force level decisions [51]. However, there was widespread agreement within DOD that how all these questions should be answered and how they were related to one another needed to be studied intensively. McNamara requested studies of the Navy and the Joint Chiefs of Staff. The purpose of the Navy studies was to develop the least cost mix of sealift, airlift, and pre-positioned supplies and equipment necessary to support a land campaign in selected geographic areas, including consideration of the level of support required for support of U.S. and Allied forces and essential civilian supplies. The JCS studies were to assess the relative military value in limited war of various rates and modes of strategic deployment, the methods of employing such capabilities to support feasible strategies, and the associated costs [52][53].

The background against which these studies was done was that the Air Force was bent on selling the CX, the JCS was calling for a mix of airlift and sealift, and the Navy was worried that a major decision on the CX was impending and that the systems analysis people in OSD were biased in favor of airlift over sealift. The Navy felt that sealift should continue to have a role in lift in spite of the emphasis on rapid deployment. The Army appears to have had little interest in the

mobility issue, although it is not clear why this is so. They may have felt the tradeoff between lift forces and Army divisions was more likely to dominate the decision than possible synergistic effects; or they may simply not have thought about it.

The JCS studies turned out to be very significant in the development of the analysis for strategic mobility forces -- not because of the force level conclusions reached or because of the methodology employed, but because of some intermediate ideas that occurred. The time-phased force requirements were calculated for each of several military strategies [54]. Alternate prepositioning modes -- varying in the amount and location of stocks -- were devised, and several alternative lift systems for meeting the time-phased force requirements of each strategy were set forth. Tradeoffs between risk and cost in choosing among the alternative lift modes were then considered [55]. The JCS studies apparently argued that a rapid deployment capability results in a preponderance of very significant advantages and that the only important disadvantage is the dollar costs of achieving and maintaining the capability. The unmeasurable deterrent value of a rapid deployment capability and the increased monetary cost of conducting a longer war involving a higher level of forces were seen as major qualitative factors that acted to cancel the cost disadvantage and thereby preclude an explicit rationale for the actual force level decisions [56].

Computer runs of RAND and RAC models apparently were not used in the study conclusions. It appears likely that these models were included largely to illustrate competence in using computers and

sophisticated models. The RAC model was a least-cost linear programming model for meeting specified lift requirements. It is interesting that the applicability of this concept to the time-phased force requirements situations developed in the main part of the study was not seized upon in 1964; as we will see later, the combination of these two ideas played a major role in the development of the strategic mobility analysis. Rather, the JCS studies seem to have emphasized that this phase of the examination was structured on a least-cost basis and that any conclusions drawn from the analysis should be tempered by the fact that a least-cost solution may not be the best overall solution. In short, efficiency in the use of resources was confused with the level of resources to be committed, and the use of a model to generate information was confused with its use to generate answers [57].

The most significant effects of the JCS studies appear to have been:

1. The development of the idea of time-phased force requirements as a basis for developing lift requirements and alternative lift systems; in the past, lift "requirements" were rather arbitrarily decided upon by negotiation between the Army and the Air Force.

2. The observation that high speed cargo ships with rapid turnaround and other provisions for rapid deployment use could be significantly useful in meeting lift requirements at less cost than a pure airlift strategy.

3. The observation that the CX could be fitted with high flotation landing gear to permit operation into airfields near the battle line rather than well to the rear.

The significance of these findings will become clearer later on.

The Navy studies were performed by a non-profit civilian organization, the Center for Naval Analyses. Preliminary results of these studies noted that the tying up of transport ships in MSTS peacetime use and the long steaming time from CONUS ports to trouble areas were major limitations on sealift in rapid deployment uses. The "Sealog" concept that came out of the study was seen as a means of overcoming these limitations. The basic concept was to abandon the "economic" peacetime employment of ships as cargo-haulers and, instead, to set them up as a readiness force whose cost was to be viewed as the price of a rapid-deployment capability [58].

The Sealog idea was to have some of the Sealog ships pre-loaded and deployed forward to provide early deliveries. Other ships would steam to the battle area in time for mid-term deliveries. Unloaded

ships would then travel to prepositioned stocks or back to CONUS to reload for sustaining deliveries. Army troops were to be airlifted to the battle area to marry up with the equipment delivered by the ships. The essential difference from the FFD idea is that Sealog ships were not to be humidity controlled and were therefore to cycle back to CONUS periodically for removal and maintenance of the equipment; also, they were to operate with the fleet in time of emergency.

The methodology used in the Navy studies was to calculate the lift forces required to deploy given force levels to counter a Chinese attack on Southeast Asia under two levels of funding beyond the funds already committed. A major source of bias in these studies seems to have been the use of a threshold type of objective function. Airlift clearly gets the first forces to the battle area faster, but it was possible to conclude that the required forces could be delivered without airlift within 30 days (the rule-of-thumb time frequently used in discussing the initial phase of a campaign) [59]. This ignored the benefits of rapid deployment in the earlier phases of the operation -- an important part of the problem.

The 1964 airlift-sealift memorandum had these two studies as an input. However, an overall structure for looking at lift decisions still had to be found before the analyses could firmly relate all the actual decisions that had to be made. Technological improvements in the Ro/Ro ship combined with the idea that it would replace the existing less efficient Victory FFD ships in the FFB role led to the decision to go ahead with a sizable production program for these ships.

Improvements in engine design reduced uncertainties about the feasibility of the CX jet transport, and the results of the breakeven analyses done in the previous year were now used as the rationale for curtailing the C-141 program and initiating the development of the C5A [60].

Both the Navy and the JCS studies provided some insights to the "right questions," but neither provided a fully satisfactory overall analysis. None of the analyses available then addressed in a definitive way whether the mix of airlift, sealift, and prepositioning which it examined was, overall, the least-cost solution to the time-phased deployment requirements. It was recognized that the problem was complex, that a general methodology for its solution had not been developed and that there was no real consensus even on how this might be done conceptually. The proper course of action in the face of this conceptual uncertainty was to plan force levels as a hedge against this uncertainty until a better idea of the optimum mix was found or until actual hardware had to be procured. The analyses <u>were</u> useful in suggesting that the hedge was a desirable course of action and what proper intermediate force level decisions were [61].

At this point in time, roughly the latter part of 1964, there was considerable uncertainty about the conceptual basis on which the necessary decisions <u>should</u> be made. In spite of the fact that the systems analysts felt they were getting close to some of the right questions, they did not foresee how they were going to answer them.

Yet the basic components for a comprehensive rationale were all there as were the components for the final decision on the FDL ships which had not even been mentioned yet.

First of all, the statement of time-phased force requirements made least-cost linear programming models relevant and usable. The particular force requirements developed in the JCS studies were not inviolable, but just stating them gave a place to start on the analysis. Realizing that the linear programming model, when applied to the scenarios and time-phased requirements, could give useful outputs in the form of optimum lift force mixes for various strategies was a long time in becoming accepted. But this approach got around the problem of simultaneous determination of combat force size, overall lift force capability, and the mix of modes within that capability. There is no visible reason why this realization took so long to become accepted.

Secondly, the confusion over the relationship of Sealog, the FFDs, and the Ro/Ro was ready to be resolved. The Army refused to accept the idea of depot ships that were not humidity controlled, and there was pretty general agreement that in spite of the C5A decision that sealift would continue to be an important part of our lift forces. Therefore, the Navy was not forced to argue for the details of the Sealog concept to assure its continued role. There was widespread agreement that some kind of large, high speed depot ship with some form of Ro/Ro features should be developed with the detailed design to

be worked out in the design phase; and details of the design and the operational concepts were no longer viewed as policy alternatives.

Finally, the C5A program was firmly programmed, and the high flotation landing gear for forward operations was proved out. Somewhere the idea of tandem use of the C5A and the depot ships had emerged. This would involve deployment of initial forces from CONUS via C5A, followed by shuttling of the C5As between the depot ships and the front lines, thereby eliminating the delays of ground transport in the theater.

At this point there was some jockeying for position, so to speak. The Navy still felt predisposed toward the Sealog operating concept and the Sealog ship design, while OSD was beginning to see the floating depot concept as more desirable. The name of "Fast Deployment Logistics" ships was selected in part because of its neutrality relative to these differences, and the FDL was born. The Navy also began a study of the Sealog concept vs. the FFB concept in order to keep the Sealog option open. Once the Navy knew pretty well what it wanted, however, it moved to coordinate with the Army on the design and operational use of the ships. They apparently felt it was not appropriate to work closely with the Army before having developed their own position. This had the effect of making the Navy relatively insensitive to Army needs, but there is no indication that the Navy meant this to be so. Rather, it probably reflected simple insurance against the possibility of being burned by getting caught without their homework done.

There was some diagreement over the FDL within the Navy. Many admirals felt that there was a historically determined shipbuilding budget and that the major program being considered for the FDL would cut into the funds available for combat ships. The lack of continuing Navy support for Sealog probably confused the concept within the Navy. The Navy clung to the Sealog use concept at least through 1965 (but not very adamantly), and it was finally so compromised with the floating depot concept that it really didn't matter.

The three major developments that contributed to the analysis that made possible a decision to proceed with the FDL program were:

1. The use of time-phased requirements and a least-cost transportation model to calculate efficient lift force mixes.

2. The concept of sealift as a readiness force for rapid deployment rather than a cargo capability for use in peacetime as well.

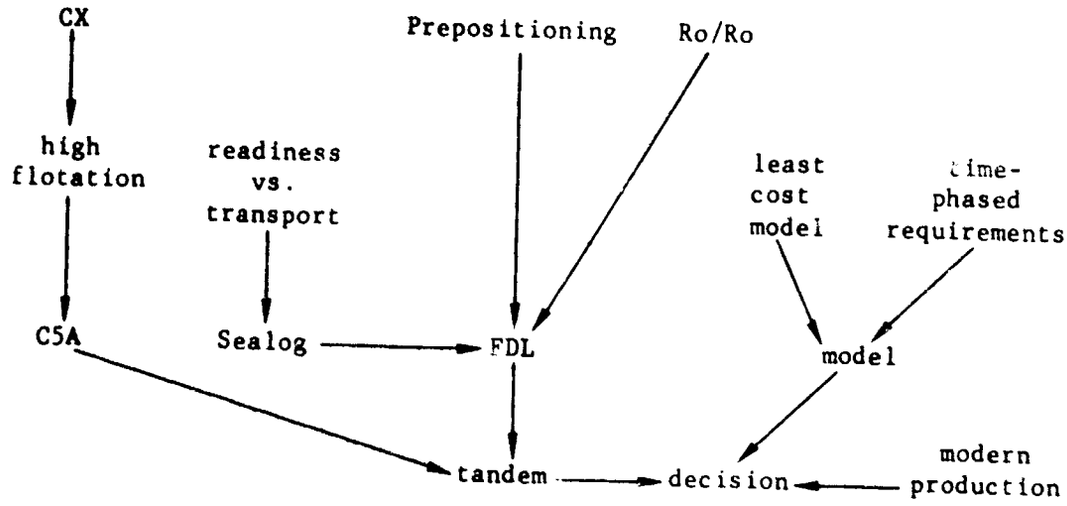
3. The elimination of competition as major policy alternatives among the various types of logistics ships and their precise operational concepts and the shift of these questions to the design process.

The idea of tandem use of the FDL with the CSA made the FDL more attractive in the least-cost model and suggested a larger program of FDLs than would otherwise have been the case, but it seems likely that the FDL would have been procured even if this idea had not been developed.

Rather than talking around the conceptual nature of the problem, by 1965 the analysis could suggest pretty firmly the mix of forces that would be desirable in terms of the capability provided and the efficiency with which it was obtained. Rather than getting bogged down in the merits of a least-cost model as the basis for decision, the analysis was able to use the model as a tool for performing sensitivity analyses with respect to significant factors such as reduced costs of FDL production, loss of vulnerable prepositioning sites, and the speed of deployment desired.

Another factor that contributed to the FDL decision was the suggestion that modernization of shipbuilding practices along the lines used for aircraft procurement would yield important cost savings. A study conducted in OSD suggested that this was indeed feasible and could be applied to the FDL program. The Secretary of the Navy was made aware of the study and proposed that this concept be applied to the FDL. Mr. McNamara enthusiastically concurred [62].

The following chart shows the major developments that led to the analysis behind the FDL decision. It is clear that in this decision the analysis was an important but not dominant factor.


CX
high flotation
C5A
readiness vs. transport
Sealog
Prepositioning
Ro/Ro
FDL
least cost model
time-phased requirements
model
tandem
decision
modern production

CHAPTER VII

Conclusions

It is very difficult to come to any firm conclusions in so amorphous and complex an area. It would certainly be tenuous to make any general statements on the basis of a little theory, a little experience, and two case histories. On the other hand, there are some interesting comparisons between the ideas developed in Chapter IV and the observations of the decision processes in Chapters V and VI. These comparisons also suggest some hypotheses about how systems analysis and organizations interact that did not seem so likely before these two observations, and that are somewhat different from the general picture that is suggested in Chapter IV.

We will first discuss each of the two cases against the ideas developed in Chapter IV. Following that, we will discuss what parts of those ideas were and were not borne out in these two cases, and suggest some more general hypotheses than those in Chapter IV.

A. CVA-67

Probably the most obvious feature of the CVA-67 decision is that the Navy either did not understand what Mr. McNamara was asking for, or was exceedingly clever at pretending they did not while trying to weaken his position through a firm (stubborn) position of their own. To argue either of these extremes would be unfair to the Navy. They must have grasped the significance of his questions, but it is doubtful that they saw how to answer them in the way Mr. McNamara had in mind. It is probable that they dealt with them as best they could, but that they were unable to link them together to justify their position. Instead, they made the leap of faith by "judgment" to a conclusion they felt at ease with and that could be agreed upon within the Navy. We must assume that the Secretary of the Navy and the Chief of Naval Operations were sincere in their belief that this judgment was proper and that it was in the best interests of the country to spend the extra money for the benefits of nuclear power.

Even if we accept the sincerity of the Navy on the merits of the decision, however, it is reasonably clear that they were not so sincere in arguing their case to McNamara. The cost-effectiveness study which cited 21% more effectiveness for 3% more cost just could not have been accepted by any reasonable man as a basis for a decision of this magnitude. The only available explanation for their willingness to send that study to McNamara is that they felt they should give him what they thought he wanted: numbers from a "cost-effectiveness study".

But they must have expected either that he would not look very closely at the methodology or that he would regard the methodology to be secondary in importance to the fact that the analysis reinforced the weight of their judgment. That Mr. Korth was so willing to rely on a consensual judgment for such strongly worded recommendations suggests that he expected McNamara to accept his judgment that the decision to go nuclear was a proper one. It also suggests that he regarded the cost-effectiveness analysis as one of many factors to be weighed in making a judgment about the proper decision rather than as a framework within which his judgments could be expressed and which formed the primary basis for decision.

This view is confirmed by the Navy's reaction to the CNA NAVWAG 28 study. A study that did not agree with the consensus of past studies and with the collective judgment (of the dominant coalition) clearly was not a proper study in their eyes; in short, decision-makers are to decide, studies are to justify. Judgment and decision appear to be synonymous to them. This corresponds to the observation in Chapter IV that decision-makers may prefer to express their judgments in lumped form at the end of analysis rather than as inputs to the analysis on specific issues. In the absence of much structure for the decision, this form of judgment is probably more reasonable; but reliance on this approach can also limit the usefulness of an analysis by discouraging active interaction with the judgment of the decision-maker during the analysis and by making the analysis more closely constrained to *a priori* assumptions and preferences.

Because there was so little structure developed for discussing the issue, much of the "judgment" or "decision" that nuclear power was best rested on analogies or rules-of-thumb that had come to be regarded as general principles: Just as sails had been replaced by coal and coal had been replaced by oil, so the technologically superior nuclear power would replace oil. When an argument like this is well made, with the weight of history and the cumulative experience of The United States Navy properly thrown in, the progression can be made to sound almost inexorable. When the decision is set in such a context, it is almost impossible to question without appearing just a little niggardly and just a little against Progress.

Another "general principle" used to justify the judgment was the "For-want-of-a-nail..." principle -- although it was never called by that name. Experienced carrier captains can -- and of course did -- cite situations where a slight margin in performance has had or could have considerable payoff. Providing for these unforeseen possibilities is very important to the carrier captains. But it is less so to a Secretary of Defense who must be interested in the <u>expected</u> payoff of increased performance characteristics. He must be willing to pay a premium for improved effectiveness, but he also must ask how much of a premium is justified. If he did not, we would be in danger of excessive application of the principle that "our boys deserve the best we know how to give them". Indeed, one reason Mr. McNamara was so adamant on the CVA-67 was his feeling that the trend to larger and more expensive ships was in danger of pricing the Navy out of business.

The use of these analogies is cited not to denigrate the Navy, but to point out the kinds of considerations people have to fall back on when they cannot (or do not) supply some structure to their decisions. If the Navy appears to have been on very weak analytic ground, OSD was not much better off. They were able to point to some of the right questions, but because of the limited systems analysis staff they could not produce a full analysis in time for the decision.

It seems clear that McNamara's insistence on an explicit rationale under these circumstances would inevitably serve to embarrass the Navy. On close examination, their decision would be shown to rest only on intuition, and they must have sensed this. Their realization that this was so was probably more implicit than explicit, and probably contributed to their reluctance (and their inability) to do much good analysis.

Commitment seems to play several roles in this decision. The most obvious is the Navy's commitment to nuclear power. But it was not a simple a priori bias; it was a commitment that grew over the course of the decision process: At each of Korth's urgent recommendations for nuclear power, it became more difficult for him to back away without calling into question both the soundness of his previous strong recommendations and the reputation of the Navy. McNamara, on the other hand, became increasingly committed to getting a good analysis before making the decision.

In addition, McNamara must have known that a decision for nuclear power would not have encouraged continued analysis of the more general policy issue. By deciding for conventional power, he in effect harnessed the energy of the Navy's commitment to nuclear power to assure that the larger question would receive intensive attention. In spite of all his potestations to the contrary, the CVA-67 decision had strong policy connotations by virtue of precedent. Had he decided for nuclear power, there would have been intensified pressure from the Navy and Congress for nuclear escorts "to take advantage of the nuclear power of the carrier". And on the propulsion decision for CVA-68 he would certainly have been asked why he approved nuclear power for CVA-67 if he was questioning it for CVA-68. In short, by deciding for conventional power, McNamara (1) avoided the commitment of precedent on future carriers and (2) harnessed the Navy's commitment to stimulate the analysis he wanted.

In summary, one would have to say that analysis on the propulsion for CVA-67 played a major role in the decision process even though it did provide a full answer for the decision problem. Analysis gave McNamara a list of questions about structure that served to sharpen his thinking and to undermine the strength of the Navy's forceful position. Preliminary results of the NAVWAG 28 study were pretty convincing to McNamara that task force effectiveness was rather insensitive to the propulsion mode. And a major result of the attempts to find the structure underlying the decision was to strengthen his intuition that the CVA-67 decision was not all that

important compared to the policy issue. A decision ignoring organizational factors, however, could have been justified either way, depending on one's predisposition: conventional power saved money, would not have to go through Congress again, and did not preclude future nuclear ships; nuclear power provided a hedge against future requirements, provided R&D benefits as a side product, and hedged against a future decision to procure an all-nuclear attack carrier fleet.

A postscript to the CVA-67 issue is the subsequent decision that CVA-68 and all subsequent carriers would be nuclear powered. It would be nice to be able to report that this was based on an analysis structured around some of the questions Mr. McNamara raised in 1963, since those questions are closely tied up with the relationships between performance parameters and the effectiveness of the carrier forces. Unfortunately, the major analysis done subsequent to the CVA-67 decision, CNA's NAVWAG 33, was not a well focused study. It treats in some detail operational differences between nuclear and conventional task forces, emphasizing in particular the relative requirements of nuclear and conventional task forces for replenishment of fuel oil, jet fuel, and ammunition. (This study did provide some insights into the augmentation of conventional task forces that would be required to make their performance approach that of nuclear task forces, but it did not really get at the central issues of the decision.) New technology also was developed that permitted a two-reactor plant for CVA-68 at lower costs than the four-reactor plant considered for CVA-67.

Mr. McNamara had stated in Congressional testimony that he felt a premium of $25 to 50 million for nuclear power was justified. Cost studies for CVA-68 showed that the cost differential with the new reactor system and with "reasonable" augmentation of the conventional task force with oilers was in a range that included this $25 - 50 million figure.

B. FDL Ships

Organizational factors appeared to be much less important in the role systems analysis played in the FDL decision than with the CVA-67, but they were present. (It is probable that these types of factors would be more evident if the sequence of memoranda were available as they were for the CVA-67 decision.) The Navy studies were motivated by apprehension that sealift would be replaced in importance by airlift, and set out to prove the usefulness of sealift. Had they not been so biased in their conclusions, this would have been a useful role for analysis. It seem quite probable that the systems analysts in OSD were somewhat prejudiced in favor of airlift because of the importance they and McNamara attached to rapid response. When analysis and intuition coincide, it is frequently useful to have the opposite case argued by an outside party, and the Navy's commitment to sealift provided the energy necessary to force the issue to the attention of OSD and the Defense Department as a whole. Even if the analysis was not particularly good, it forced OSD to look at the issue more intensively.

The other major organizational factor in the evolution of the FDL decision process was the lack of unanimous support for these ships within the Navy. As we noted in describing that decision process, many of the admirals could not be convinced that there was not some implicit shipbuilding budget that would bring about reductions in combat ship construction in the event of so large an FDL program. The effect of this division was neither to stimulate generation of new alternatives within the Navy nor to preclude the Navy's commitment to the Sealog operating concept and the Sealog ship design; its effect was to soften the force of that commitment. This was probably the major reason for the shift of the ship design parameters and the detailed operational concept away from their original status as major policy alternatives to a role of secondary importance (i.e., from the strategic planning function to the management control function).

The most interesting feature of the FDL decision process is not the impact of organizational bargaining on the analysis, but the way in which alternatives and analytic structure evolved over the process. The analytic framework that evolved has been one of the major successes of systems analysis in the Defense Department -- in the sense that it provides a rationale for decision that is convincing and is accepted in its fundamentals by the services as the framework within which the rapid deployment force structure decisions should be made.

(It is not clear whether this acceptance would have been forthcoming had the analysis not concluded that a mix of air and sealift was called for. As it was, none of the services was very unhappy with the results of the analysis, so there was no great incentive to question its methodology.)

The evolution of the decision is marked by three particularly important points:

(1) the concept of viewing sealift as a readiness force rather than a transportation commodity,

(2) the idea of tandem operations of the FDL and the CVA,

(3) the factoring of the analytic structure into time-phased force requirements and least-cost mixes for meeting those requirements.

Each of these represents an improvement in the structure underlying strategic mobility decisions, and the first two also represent qualitatively different alternatives. As is typical of structural breakthroughs, no analysis was required to realize them; the analysis did not lead directly to their realization. Once thought of, they are obvious. But it can be exceedingly difficult to think of new ideas before they become obvious. This relates to the discussion in Chapter IV

about cognitive limitations, and particularly how ideas and concepts get accepted or "settled-in" into the way individuals think about a problem and the types of arguments the organization accepts as persuasive.

We really know very little about how these types of ideas come about. Probably the most constructive thing that can be said is that someone was able to move to the next higher level in the hierarchy of the problem and through something like what Miller, Galanter, and Pribram call meta-Plans to gain some insight into the structure. Although we cannot conclude that analysis contributed directly to these ideas, the evolution of the FDL decision does suggest that having people look for structure and for alternatives makes such ideas more likely to happen.

As stated in Chapter IV, it is useful to have people in the organization who learn meta-Plans rather than Plans or programs. Such activity is highly uncertain in its payoff, however, and the groping around involved is hardly in agreement with the popular image of systems analysis as a computer-based, highly sophisticated, precise science. And it runs a high risk of failure to produce results that are useful in the decision process. (The search for the "right questions" in the case of the CVA-67 decision produced little in the way of results and, in fact, probably contributed to pushing the Navy even farther into their commitment to nuclear power.)

The benefits of a success in this kind of analytic activity can, however, be considerable. By 1966, some of the services were able to concur fully with the general approach of the OSD analysis and to agree to apply the analysis in their differences with OSD in order to resolve them. (It is interesting to compare this attitude with that reflected by the Navy's comments on the NAVWAG 28 study in 1963.)

C. Reassessment of Chapter IV

One of the major problems with a study such as this is that it is next to impossible to disprove hypotheses, while it is possible -- because of the great complexity of the phenomena being studied -- to find some evidence for almost any reasonable propositions. With this in mind, the general conclusion reached as a result of the examination of the two decisions is that the phenomena postulated in Chapter IV are reasonably consistent with actual experience, but the decision process is much more diffuse (less sharply defined) than suggested by Chapter IV.

In particular, the uses of systems analysis in structuring the decision, suggesting alternatives, clarifying objectives, exploring ends-means interactions, and evaluating alternatives are all very real. But in a predominantly unstructured environment, people can rarely say, "Now I shall clarify my objectives;" that just is not a very operational statement. Clarification of objectives is tied up with the generation of alternatives, with all the other uses of

analysis, and with the bargaining environment. Because it is so difficult for those involved in the decision process to get a handle on a very amorphous situation, these uses appear primarily in retrospect. That is to say, attribution of motives after the fact in very unstructured environments is likely to lead to a sharper and much more purposeful picture than was actually the case during the decision process.

It is well known that "science" is not very scientific. That is, the process by which fundamental discoveries and advances are made is typically heavily weighted with serendipity. The extensive education of the scientist no doubt makes him more capable of recognizing the significance of his findings and makes him more likely to find useful results, but the process by which he does so is quite unstructured thinking. So it seems to be with analysis. The analytic capability can be expected to have some useful results, but it is difficult to plan what they will be in a specific decision situation. Both systems analysts and scientists have the tendency to present their findings in a linear deductive form that masks the process by which they reached their conclusions and emphasizes directed behavior. We must be careful not to expect scientists, systems analysts, or decision-makers to behave in the precise way they often suggest that they do.

This same amorphous quality of the decision carries over to the bargaining process. The two cases studied suggest that people are less Machiavellian than they might be if they understood better what

the decision was about, what their objectives were, and how to use analysis to evoke the desired responses from others. They also suggest that people like to think of their personal decision processes as sincere and balanced; to use analysis explicitly for confusing, embarrassing, or some of the other purposes suggested in Chapter IV would not be consistent with this view. Often, it is more likely that such motives are attributed in retrospect by other participants in the decision process. Just because people act from sincere and honorable motives, however, is certainly no reason to expect that the effects of their actions will be perceived by others as altogether positive.

In short, it was probably an oversimplification to expect to be able to identify specific uses and limitations of analysis in the on-going decision process of an organization. When both the decision and the bargaining environment are very poorly structured, as is often the case in strategic planning, people can seldom identify specific uses for analysis -- in their thinking about the decision or in their bargaining strategy -- because of their uncertain perceptions of where analysis will have some success and what results it may produce. We should, therefore, expect the behavior of the participants in the strategic decision process to be more nearly a grasping of whatever of those premises identified by analysis that seem useful, and less a premeditated plan for achieving well-defined purposes.

D. Analysis and Organizational Behavior

It is clear by now that there are many interactions between analysis of a strategic decision problem, the cognitive limitations on the participants in the decision process, and the bargaining environment that characterizes the strategic planning function in an organization. The best way to organize a theory of how these interact appears to be to list a number of characteristics of the decision process and then discuss how analysis and its uses interact in terms of each characteristic.

The concepts we have discussed can be summarized into thirteen characteristics of the strategic decision process in an organization. They are:

1. Structure
2. Incrementalism
3. Relativism
4. Simplification
5. Alternatives
6. Commitment
7. Energy
8. Expectations
9. Motivations
10. Information
11. Coalitions

12. Rationality

13. Improvement

The conceptual uncertainty about the structure underlying a strategic decision and the search for improved structure through explicit analysis are responsible for much of the interaction of analysis and organizational behavior. This is because the perceived structure (as the set of relationships among ends and means) has a strong impact on what is seen as acceptable rationale for a decision and consequently on the defensibility of a particular choice. By emphasizing structure and the information required to link ends and means, systems analysis can have the effect of either increasing or decreasing the confidence the decision-maker subjectively feels in the appropriateness of his choice. By supplying a firm foundation and rationale for a choice, it can increase his confidence; by pointing up sharply inadequacies in the structure and many of the uncertainties, it can decrease his confidence. Similarly, analysis can act either to reduce or to increase the conflict among various sub-organizations and coalitions on a decision. To the extent that it educates the various participants and supplies a common framework for discussion, it can reduce the area of conflict and provide a mechanism for resolving the residual conflict. But it also can have the effect of sharpening the differences the participants perceive between themselves and others, thereby increasing the conflict in the organization.

Because the structure brought to bear in a decision reflects only a small part of a highly interconnected system made up of the organization and its environment (and even that small part is only partially certain), it is necessary for decisions to be limited to some relatively nearby neighborhood of current policy and current coalitions. This means that the decisions are incremental in terms both of policy and bargaining. This is reflected in systems analysis methodology by its use of partial ad hoc models and suboptimizations. By improving the structure underlying a decision, systems analysis may enable larger increments to be made confidently; but it may also cause some decision-makers in some situations to be more conservative because the limitations of available information are more clearly seen.

Because it is not feasible for the organization to trace its goals back to first principles or to stable explicit goals that are sufficiently operational to serve as criteria for decision, and because structural uncertainties can seldom be resolved fully in time for decisions, the decision process is characterized by relativism of goals and rationale. This is reflected in systems analysis by the use of proximate measures of effectiveness and the emphasis on sharpening the judgment of the decision-maker, rather than on a completely rigorous deductive proof of optimality. Models are developed only far enough to make a convincing case, and what constitutes a convincing case is based more on achieving some kind of local equilibrium of the most pertinent considerations than on tracing out all the relationships between the decision and the rest of the environment.

Because of the complexity of the decisions and the need to make choices without fully resolving real conceptual uncertainties, it is essential for the participants in the decision process to simplify in dealing with decision problems. This is done in several ways, some of which have been discussed above. In particular, sequential attention to goals and to ends-means relationships enable choices to be made in the presence of uncertain and inconsistent values and structure. Decentralization and suboptimization permit choices without a fully developed and interconnected structure. This is reflected in systems analysis through ad hoc models, ad hoc attention to issues, and sub-optimizations. Simplification may facilitate the bargaining process by glossing over potential differences in rationale and values, or it may be a source of conflict because different people simplify differently and the simplifications are not made explicit.

Alternatives play a central role in the decision process. The end point of the decision process is, after all, to find high quality alternatives for implementation; evaluation, structure, and goals are only intermediate devices for achieving that purpose. This is reflected in systems analysis through its emphasis on finding alternatives that are better than existing ones rather than searching for some objective optimum. Alternatives provide the basis for ends-means relationships and thereby are the vehicle by which objectives are formulated and the structure is clarified. Knowledge about structure implies knowledge about alternatives. Qualitatively different alternatives are generated in systems analysis by uncovering a better structure for the decision.

Such new alternatives are one of the most useful results of analysis because they in a sense redefine the problem, can be communicated simply, and relieve commitments and predispositions for old alternatives.

Commitment to alternatives during the decision process seems to be an inevitable result of the intertwining of objectives and alternatives. The generation of alternatives requires screening out of the better ones, and the individuals or sub-organizations that generate alternatives naturally develop ideas of what is best for the organization as they develop alternatives. This tendency to premature commitment (before the decision structure and all the interesting alternatives have been developed) can be a positive or a negative influence on the decision process: it may prematurely exclude or prejudice good alternatives or objectives, or it may act to assure that good alternatives will receive consideration that they otherwise would not.

Energy in the strategic decision process is basically generated by support and opposition of alternatives. Objectives and structure are of concern only as they influence what alternatives are considered and chosen or as they indicate directions for future choices. Systems analysis has concentrated on avoiding tendencies toward premature commitment and has not developed any methodology relevant to the concepts of energy and commitment as useful components of the decision process. In particular, suborganizatic · require some kind of reward

for investing the energy to develop an alternative and bring it to the point of active consideration as a major policy alternative in the decision process.

Expectations about the usefulness of analysis and its consequences for other participants in the decision process are only partial and near-term. They are partial in the sense that they relate to only a few of the considerations that are relevant, and they are near-term in the sense that they consider primarily only immediate effects and reactions. This is reflected in the cut-and-try approach of systems analysis in evolving structure, goals, and alternatives rather than attempting to proceed by direct deduction to the answer. It means that the analysis in the strategic decision process is more nearly characterized by serendipity and recognition of useful analytic results than by straightforward production of intended results.

Because of the limitations on expectations, motivations of the participants for their actions during the decision process are similarly tentative. In the presence of considerable structural uncertainty about the decision and/or the bargaining environment, these motivations will be undifferentiated; the participants will proceed more by "feel" than by conscious design. This will be reflected in the uses people see for systems and analysis and consequently on the type of analysis that is sought rather than on the methodology of systems analysis. Because motivations and expectations are based on information received and on its interpretation, they are dependent on the individual's role in the organization and on his previous experience.

Information in the organization is neither uniformly distributed nor uniformly interpreted. By using systems analysis as a common framework for discussion, these differences can be reduced or at least made explicit. It can also point up what kinds of information are useful for the strategic planning function.

Rationality in strategic decision-making is contingent on the context of the decision and must be broadly defined. Approximate rationality is a more useful concept than strict deductive rationality. Because only the more important considerations are explicitly considered in the rationale for the decisions, rationality within the boundaries of a specific decision problem is a limited concept. The many factors outside the boundaries of explicit consideration are a component of rationality in decision. An outside observer may conclude that many decisions are irrational if he considers only the factors that were made explicit. Because of the conceptual and other uncertainties of strategic decisions, rationality must be interpreted more in terms of avoiding gross errors than of optimization, and more in terms of comprehension than of deductive logic. Systems analysis reflects this need for a broadly conceived rationality by an ad hoc approach, by emphasizing the sharpening of the decision-makers' judgments, and by going only as far as is necessary to build a convincing case.

Finally, the decision process is characterized by the desire for improvement. Where performance standards have been established, as in day-to-day operations, satisficing is appropriate behavior because alternatives need be considered only until the standards are met. But

in the strategic planning function, it is desired to find -- at a given point in time -- the best alternative that is available. The difficulty of finding qualitatively different alternatives and the pressures for premature commitment mean that only a few alternatives will receive explicit attention in the final choice process. Observations of this from outside the organization may mask the intent to optimize and suggest that satisficing behavior applies, when in fact it does not. This desire for improvement means that the seriously considered alternatives will be approximately Pareto-optimal (given uncertainties about structure and objectives). Systems analysis is simply a systematic way of pursuing this improvement at the point in time when a choice must be made. To the extent that analysis produces new alternatives that are widely interpreted as improvements, it will not react adversely with the interpersonal interactions in the decision process. The constraining impact of organizational behavior characteristics on analysis (and vice-versa) are due to the explicit setting out of rationale that people cannot agree will lead to an improved alternative.

It would be nice to be able to present some normative principles for how to perform and use analysis in the broader decision process. All the considerations discussed above lead, however, to the conclusion that it is not feasible to develop highly specific principles. The primary reason for this is the extreme contingency of the decision process; principles that are specifically operational rather than vaguely "true" cannot be developed with our present understanding for the wide range of circumstances that can -- and do -- arise. The

normative value of studies such as this is that the descriptive insights can be assimilated by analysts and decision-makers to improve their "feel" for situations. It can enrich whatever it is they do, but it cannot prescribe fully just what it is they should do. In spite of this disclaimer, this is a valuable result and it is worth listing a few principles:

1. Analysis for strategic planning is an active and creative function, not an academic discipline. The criteria for a convincing case in a decision are evolved during the decision process, and they can in part be consciously shaped by the participants as the process evolves.

2. In unstructured situations, bright and easy to understand ideas about structure and about qualitatively new alternatives have more payoff than evaluative precision in comparing known alternatives; having people involved in the decision process who are used to trying to bring structure to decisions and to devise qualitatively improved alternatives can have a considerable payoff.

3. Imposing strict systems analysis procedures for justifying decisions within the organization can act to inhibit the growth of alternatives by forcing people to justify decisions in a language only

slightly related to the process by which they reach decisions, and by dampening their enthusiasm for developing a potentially useful alternative.

4. The energy and enthusiasm associated with commitment are an essential and important part of the decision process that must be considered along with systems analysis and program budgeting concepts in developing an effective decision process for strategic planning, and that should be considered by those acting in the decision process.

CHAPTER VIII

Implications for Future Research

Suggested research topics at the end of a study are frequently of little value for two good reasons: they are seldom well enough thought out to represent a reliable list of relevant studies, and they ignore the viewpoint the reader will bring to the problem. It is hoped that some of the ideas and observations in this study are sufficiently interesting and thought-provoking that others will pursue the same general topic from other vantage points. In this chapter, therefore, we will only present some implications of the experience of this study for problems others might have.

A major question is the goal of the research. Some have approached management research in the same spirit as research in the natural sciences: attempting to describe -- and hence to predict -- through an efficient set of principles and relationships. Others have started from what seemed well established principles and attempted to optimize over the limited set of factors assumed independent. The uncertainties of unstructured decision situations is such that prediction is not practicable except for broad tendencies. These can be useful in understanding some of the outward characteristics of decision-making

in organizations, but will not be of much value in predicting behavior in individual situations. It is not likely to be of much value, then, in the practice of management. The problem with the latter, optimizing, approach has already been discussed: In so complex an environment we can rarely optimize in any very global sense; rather we must learn to improve within the cognitive, psychological, and organizational limits of our capabilities.

This suggests two goals of research into management decision processes from a normative standpoint. The first is that cited by Cyert and March: to view the organizational strategic planning process as an adaptive system and to seek to manipulate its characteristics in order to improve the general quality of the decisions it makes over the long run. (Of course quality in this context includes the avoidance of any gross failures and not simply an expected value.)

The second suggested goal is to give the manager some insights into causes of organizational and individual behavior so that he can be more aware of the implications of his actions and those of others. In particular, it does not seem a feasible goal at this time to provide the manager a theory to predict what others will do in particular situations. But we can give him insights that enable him to learn about the other participants' views, motivations, and rationale as he interacts with them, and thereby to function more effectively in the organizational decision process.

The two major problems in this type of research seem to be the lack of a specialized vocabulary and the necessity for making tentative conclusions based on slim evidence. The former difficulty is a real one. Common words such as "decision", "judgment", "structure", etc., cover a wide variety of concepts. In particular, the difficulty takes two forms: these common words have different connotations to different people, and there are only a limited number of relevant words to cover increasingly differentiated concepts. We have found it very difficult to develop very incisive relationships in the face of this. Inventing new words in the social sciences is much more difficult than in the natural sciences. (This "difficulty" is at least partially a boon: by letting each person read his own connotations and associations into these words, agreement is more readily forthcoming than it would be if we were able to be precise.)

Conclusions in a study such as this are not based solely on the initial hypotheses and the observations. They are strongly shaped by the author's past experience in participating in situations similar to those being discussed. This makes the link between hypotheses and conclusions especially uncertain, and suggests that those without such experience or with somewhat different experience in similar types of situations may not agree with the conclusions drawn.

Peter Drucker [63] has said:

> Some things a man can learn before he becomes a manager; he can acquire them as a youth or as he goes along. Others he can learn only after he has been a manager for some time; they are adult education.

This is not an unreasonable conclusion. We have argued that theory and principles in so complex an area are at best incomplete. To someone without experience in the types of situations we are talking about, the theory will seem hopelessly simplistic and naive or needlessly complicated. But to someone with such experience, it can provide a background framework around which he can organize previously unorganized ideas; to such people the terms can become quite meaningful.

At the risk of some embarrassment to the author, it is probably worthwhile to compare the proposed procedure of this study with the final result. In particular, the analysis of the two decision processes was to:

1. Classify the characteristics of systems analyses:
    a. Common and differing assumptions
    b. Judgments on fact and value, explicit and implicit
    c. How alternatives arise
    d. Techniques, rules of thumb, heuristics, and intuition
    e. Structuring or evaluating

2. Classify the uses, both stated and apparent, of analysis
    a. Structuring the problem for discussion
    b. Providing inputs to choice
    c. Goal clarification and definition
    d. Bargaining material

3. Classify the limitations, both stated and apparent, of analysis
    a. Imperfect representation of the situation
    b. Inappropriate models
    c. Procedures for dealing with multiple and overlapping goals
    d. Imperfectly defined objectives
    e. Inadequate data
    f. Computational constraints
    g. Unresolvable uncertainty
    h. Interpersonal conflict

This is not -- in retrospect -- a particularly useful structure for organizing observations in an extremely unstructured decision situation. It is in a sense much too precise. People cannot agree, for example, what are assumptions and what are facts. Because of differing views and implicit structuring of the problems, one man's assumption is another man's fact and still another man's variable. Judgments about fact and value are seldom differentiated from one another. The application of techniques and rules-of-thumb can to some extent be identified, but their significance relative to one another and to other aspects of the decision process cannot be readily assessed because it is not assessed by the people who use them.

Similarly, the uses of analysis are very hard to separate from the analysis itself. In any given situation, one can never be quite certain to what extent which of two explanations applies: whether (1) the analysis is being used to achieve some direct or ulterior effects or (2) those effects are being incurred incidentally or intentionally as the price of getting the analysis done. For example, McNamara may have accepted the strife of the CVA-67 propulsion issue as the cost of getting some analysis done for the decision, or he may have insisted on analysis more in order to keep the propulsion option open for future years.

The limitations of systems analysis were more readily observable than were the uses. This is attributed to the difficulty the participants in the decision process have in setting out to achieve some specific result in a highly unstructured situation. The one exception to this is that the limitation of computational ability was not evident. The lack of structure in the CVA-67 decision made detailed computation largely irrelevant; and even in the strategic mobility model, structural uncertainties limited the use of linear programming to simple situations well within computational capabilities.

If we were to summarize the most significant implication of this study <u>for future research</u>, it would have to be that uses of analysis by participants in the strategic planning process of an organization can be attributed <u>after</u> the situation has been resolved, but seldom are explicitly foreseen by those people in deciding how they will proceed <u>during</u> the decision process.

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